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THE

# Society of Tropical Medicine and Hygiene.

## FIRST ANNUAL REPORT OF THE COUNCIL.

### Formation of the Society.

At a meeting of Medical Men and others interested in Tropical Medicine which was held in the Conference Hall of the Colonial Office on January 4th, 1907, it was unanimously resolved to form a Society to promote the study of the Diseases and Hygiene of warm climates, and to facilitate intercourse and discussion, among the Fellows. At subsequent meetings held at the Royal College of Physicians, and at 20 Hanover Square, the Society was duly constituted, Sir Patrick Manson being elected first President, and Professor Ronald Ross first Vice-President. The Council was also elected and Laws were approved and adopted.

### Meetings.

The first ordinary meeting of the Society was held on June 26th, 1907, when the President delivered his Inaugural Address, and Dr. Daniels gave an Epidiascopic demonstration illustrative of Tropical Disease. Except during August and September, meetings have been held regularly every month since that date, the average attendance being forty Fellows and visitors.

### Papers.

During the year ending March 31st, 1908, twenty-one Papers were read and seven demonstrations were given before the Society. The chief subjects of discussion were Trypanosomiasis, Kala-Azar, Oriental Sore, Filariasis, and Ankylostomiasis, while important contributions on protozoological and helminthological science were read by Drs. Stephens, Nierenstein, Breinl, and Todd of Liverpool, Mr.

Shipley of Cambridge, and Drs. Sambon and Leiper of London. A Paper on "Acclimatisation in North Queensland" was read by Dr. Macdonald, Geraldton.

### Fellows.

The following have accepted the Honorary Fellowship of the Society:—

Professor Danilewsky, Kharkoff.
Professor Paul Ehrlich, Frankfort a/Main.
Dr. Carlos Finlay, Havana.
Professor Camillo Golgi, Pavia.
Jonathan Hutchinson, Esq., London.
Professor A. Laveran, Paris.
Professor Ettore Marchiafava, Rome.
Professor K. Shiga, Tokio.
Dr. Theobald Smith, Boston, U.S.A.

186 Ordinary Fellows have been elected.

### Finance.

To the end of the financial year (March 31st) the income of the Society was £160 12s., and the expenditure £102 18s. 10d, a balance of £57 13s. 2d. being thus left in hand.

### Transactions.

A volume of Transactions, which will contain a full report of all Papers and discussions up to the end of July, 1908, a list of Fellows, and other information, will be published in the autumn.

Arrangements are now being made by the Council for next year, by which, in place of an Annual Volume of Transactions, Reports of the Proceedings of Meetings will be issued and sent to each Fellow monthly.

F. M. Sandwith
W. Carnegie Brown } *Hon. Secs.*

*June 19th*, 1908.

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TRANSACTIONS

OF

# The Society of Tropical Medicine and Hygiene

## INAUGURAL ADDRESS

BY THE PRESIDENT,

SIR PATRICK MANSON, K.C.M.G., F.R.S.

(*Read at the First Ordinary Meeting of the Society, June 26th*, 1907.)

I TRUST that it does not augur badly for the success of the Society of Tropical Medicine and Hygiene that its first President has to commence his Inaugural Address with an apology. Such, unfortunately, is the case. I intended to devote some time to preparation, but having been called on unexpectedly to take part in the recent International Conference on Sleeping Sickness, I have been so occupied that I have not been able to find adequate time or to give that care to preparation that the importance of this occasion demands. I apologise in advance, and trust you will be indulgent to my shortcomings.

Before proceeding further, I wish to express my appreciation of the compliment the Council has paid me in electing me the first President of the Society, a Society which, although the youngest, has some reason to expect that it will be one of the most useful and fruitful of the medical institutions of the country. During nearly the whole of my professional life tropical disease has interested me, and as opportunity offered I have endeavoured to follow up the subject, and, if I may be allowed the expression, to forward the interests of the cause. Now that I am fast approaching the end of my career, to find that these

efforts, however humble, are appreciated by my colleagues is to me most gratifying. I need hardly say that I thank you sincerely for the compliment, and that I shall do my best to discharge the duties of the office with which I am entrusted, and to forward the interests of our Society.

Our objects are various. An important one is to bring together the men who are interesting themselves in tropical medicine, with the idea that by so doing the subject itself will be advanced and we ourselves benefited. With a view to securing these objects, the Society has been placed on a basis as broad as possible. Although domiciled in the Metropolis, it is open to any member of the profession, whether domiciled in London, Great Britain and Ireland, or abroad, and also to those followers of any science or profession capable of forwarding, directly or indirectly, the interests of tropical medicine.

It is apparent to any one who has followed the discoveries of recent years that tropical medicine, more than any other branch of medicine, is dependent on several of the collateral sciences, more especially on the various branches of natural history. Our basis therefore has to be a broad one, and in drawing up the constitution of the Society this point has been kept steadily in view.

At one time the specialising of tropical medicine was looked on somewhat askance, and still more so the establishment of special schools for the teaching of tropical medicine. Doubtless the formation of this Society will be regarded by some in a similar way, but as time goes on and the general body of the profession becomes acquainted with our objects and our work, I have no doubt, as has been the case with the special teaching of tropical medicine, that we shall soon be tolerated and ultimately approved.

How important it is for the subject itself, as well as for those who are engaged in its study, that the latter should have opportunities, such as this Society is calculated to

afford, of placing themselves abreast of the times will be readily understood when we think of the rapid progress that tropical medicine has made within the last few years. This progress has been as remarkable as it has been great; indeed, it is hard to keep pace with it. No sooner have we settled down to digest some new and important discovery than a fresh and perhaps more startling one is offered us; and this, too, we must digest and assimilate if we are to practise, or to teach, or to work to the best advantage.

As with the growth of a child, we hardly appreciate the recent progress of tropical medicine, living with it, so to speak, as we do from day to day; but if we come from abroad after an absence of years, or if we compare the state of knowledge, say, of twenty years ago with that of to-day, we shall find that the stripling has not only altered in feature, but has grown into a veritable giant.

I sometimes take from their shelves the text-books of my student days and compare their contents with those of the text-books of to-day — Watson's with, say, Osler's *Principles and Practice of Medicine.* The contrast is remarkable. A perusal of Osler's shows how nearly all the old theories have been upset; how new diseases have been brought to light, old drugs and methods of treatment abandoned, pathology and etiology in most instances completely changed. If the contrast be great between the general medicine of to-day and that of forty or fifty years ago, it is even still greater in the case of tropical medicine. As regards the latter, it is unnecessary to go back to the days of Watson to recognise a striking contrast. It suffices to compare the tropical medicine of the early 'nineties as represented, say, by Davidson's *Hygiene and Diseases of Warm Climates*, published in 1893, with one of the recent text-books, say, the section on Tropical Medicine in

the new edition of Allbutt's *System of Medicine* edited by Rolleston.

Of the 1000 pages in Allbutt's book, the first 200 are exclusively devoted to protozoa, mosquitoes, blood-sucking flies, and ticks. In Davidson's work there is hardly a sentence on these subjects. Practically, this is a new and a big and rapidly growing branch of tropical medicine.

In Davidson's work the first chapter in the section on General Diseases relates to malaria. It is a very complete, carefully-written article, embracing all the most important knowledge and views of the time; yet in that article there is not a single word on the mosquito as a carrier of malaria. In those days, although Laveran's great discovery had already been before the world for twelve years, we had got no further than the conviction that the air was the common medium of infection, and there was still a lingering belief in the hippocratic idea that the drinking of marshy water produced enlargement of the spleen. Now a large part of the tropical section of Allbutt's system is occupied with the part played by the mosquito in the malarial drama. Everyone knows that the mosquito is the sole vector of malaria, and an enormous literature has grown up around a discovery which has changed radically our views, not only as regards the etiology and prophylaxis of malaria, but has given a powerful stimulus to the study of the protozoa in general, and also the *rôle* of insects in the transmission of disease germs.

Taking the chapters in Davidson's book in their order, we come next to Tropical Typhoid Fever. Since this chapter was written, Wright's bold and apparently successful prophylaxis has been introduced, and is being extensively practised. This, although not relating to a specially tropical disease, may in time be shown to be a great advance in

the prophylaxis of one of the most serious diseases occurring in the Tropics.

When he wrote the chapter on Malta Fever, Bruce had already discovered the *Micrococcus melitensis*; but, just as was the case with Laveran's discovery, we had to wait a long time before the discovery bore practical fruit. Now, however, we know that the micrococcus is acquired in what, when Davidson's book was written, was an entirely unsuspected medium and manner. Both from a scientific and practical standpoint, Zammit's discovery that the germ of Malta fever is eliminated in the milk of apparently healthy goats is a discovery of a little more than a year's standing. It carries with it important practical results, as has already been proved.

Yet more important are the recent discoveries in regard to the still undiscerned germ of yellow fever, especially its dependence for propagation on the offices of the stegomyia mosquito and the efficiency of the prophylactic measures founded on that circumstance.

We cannot claim any definite or important advance in our knowledge of dengue; but in plague, the subject of the next chapter in Davidson's book, the *rôle* of the rat in its diffusion—so familiar to the ancients—has once more been rediscovered and more soundly established than ever, this time not by observation only but also by experiment. This is a discovery that promises to be of great practical value in the struggle against a terrible disease; its establishment on carefully observed facts is quite a recent occurrence.

We cannot claim that there has been much material advance in any of the subjects treated of in the next three chapters of Davidson's work, namely, those on Cholera, Leprosy, and Beri-beri; but, as regards the subject of the succeeding chapter, Negro Lethargy, or the Sleeping Sickness, it may be claimed truly that the whole subject, both

as regards etiology, symptoms, diagnosis, and treatment, has been completely revolutionised and placed on a sound, and, it may be, hopeful, basis. We now know that the sleeping sickness is a terminal phase of a trypanosome infection — a type of disease hitherto unrecognised in human pathology—that the immediate cause of the sleeping symptom is an infiltration of the lymphatic spaces of the brain with certain small mononuclear cells, that the disease is conveyed by the tsetse fly, and that it is amenable to some extent to arsenic, mercury, and certain dyes—all absolutely recent discovery.

Turning to the next chapter in Davidson, we are obliged to remark that the battle is still in progress as to whether yaws is syphilis or an entirely independent disease. Castellani's recent discovery of a spirochete in yaws, though interesting, has by no means solved the question.

In connection with the subjects of the next four chapters of Davidson, much work has been done and is going on; but it is to be regretted that these strenuous efforts have not proved more fruitful. The tropical fluxes—diarrhœas and dysenteries—in the aggregate constitute, I believe, the most important department of tropical medicine. Unfortunately they are still, both as regards etiology and treatment, in an unsatisfactory state. It is true that the amœba is creeping into favour, and that we have got the length of recognising a bacillary dysentery; but the amœba and the bacillus dysenteriæ do not, I feel sure, cover the entire field, and, unfortunately, the knowledge of their existence, although it may have done a little for diagnosis, has not materially strengthened our powers of treatment.

Sprue remains a mystery. It is a specific disease undoubtedly, but the specific element has not been detected. On the other hand, the specific relation of the amœba to a certain type of dysentery and to liver abscess may now be

regarded as thoroughly established. This at least is a gain. Its complete recognition is of recent years.

There are still spurs to be won in this field of the tropical fluxes.

Almost equally important in their own line are the numerous additions to tropical helminthology. They are far too numerous to mention on the present occasion. I might allude to one or two of them, as they are of special interest. Chief among these is Looss's discovery that the larval ankylostome obtains access to the intestinal canal by penetrating the skin on the surface of the body—a discovery suggesting possibilities which have to be reckoned with in regard to other nematodes, or even parasites in general, when we study the route by which they obtain access to the human host. The intact epidermis can no longer be regarded as the impenetrable coat of mail it was supposed to be. Leiper's experimental demonstration that the guinea-worm may be acquired through swallowing its cyclops intermediary, and also the discovery of two new and probably important parasites, *Schistosomum japonicum* and *Amphistomum watsoni*, are important additions to this department of tropical medicine. The linking up of the larval *Filaria diurna* with its parental form *F. loa*, as well as the recognition of this parasite as the cause of Calabar swellings, is a modern event.

As regards the chapter on Skin Diseases, similar advances could be credited to recent years.

The text-book of to-day has therefore to be in many respects a very different work from that of Davidson's. Besides amplification and alteration there have to be many absolutely new additions. Kala-azar, for instance, is an entirely new chapter, dealing with a subject absolutely unknown when Davidson wrote, and it is one which will soon be found to be of far-reaching importance, both practically and theoretically. The modern text-book

must have chapters on spirillosis, seeing that the term "relapsing fever" which Murchison and others used, and which Carter also used in his studies in India, covers not one but a number of infections, spread probably by a corresponding number of previously unsuspected ticks or other blood-suckers.

Then the tropical pathologist must take cognisance of a number of diseases of the lower animals, important in themselves, but more important in their bearing as illustrating principles applicable to human pathology, especially tropical pathology. The study of the arthropod blood-suckers and the correlated diseases has grown to be so vast a subject that for its proper appreciation special studies have to be made and its teaching relegated to special men. In Davidson's day a single culex was all that we had to bother ourselves about; but nowadays we have to know something about some 600 species of mosquitoes; and so in a less degree with ticks, tsetse flies, and several other though less important blood-suckers. Truly the burden of the student of tropical medicine has become a heavy one.

I might enumerate many other discoveries and changes in our special branch. Those I have alluded to suffice to show that within the last thirteen or fourteen years the progress in tropical medicine has been phenomenal. If we are justified in looking upon the recent past as an earnest of what will take place in the near future, my successor thirteen or fourteen years hence, when he sums up the additions that will have been made to our subject by that time, will have, perhaps not a more difficult task, but certainly a longer one.

Not only has recent discovery widened our horizon, but many additional workers are now in the field, and hitherto untrodden departments of natural science having a bearing on tropical pathology have been opened by the tropical

pathologist. The multiplication of workers is extraordinary, and doubtless is accounted for by the establishment of the several tropical schools in Great Britain and elsewhere; not least by the sympathy and active encouragement that research in tropical pathology has received at the hands of the British Government. Time was when with justice we could reproach our rulers for indifference in this matter. Nowadays quite a different feeling obtains in official circles, and instead of being repressed and cold-shouldered the investigator in tropical pathology is encouraged in every possible way by the British Government and its officers. Other Governments, too, have ceased to be indifferent; some are actively sympathetic.

The interest which the British Government has shown in sleeping sickness promises well for our object. The fact that His Majesty's Government has called a Conference of the Foreign Powers to discuss this subject from an international point of view shows that we are entering on a new era as regards the Governmental element in the study of tropical disease. It may be that this Sleeping Sickness Conference is but the prelude to similar discussions and international agreements on other important tropical diseases, a matter of more importance than at the first glance it might appear to be.

All over our own tropical possessions laboratories are being established for the special investigation of tropical disease, and for the assistance of the tropical practitioner. These laboratories have just begun to bear fruit, and I have not the slightest doubt that ten years hence we will have important discoveries as well as an enormous accumulation of important data as a result of this enlightened policy.

Our *rôle* as a Society in this respect is to try and bring ourselves and our fellow-practitioners abreast of what is being done by this army of workers; so that those of us who have to teach in this country, and those of us who

go abroad to practise or to investigate, shall be thoroughly abreast of what has been done and of what is being done, and be in the best position to do full justice to tropical patients and to tropical communities, and to lend a hand at advancing our subject.

That the Society will work, and work successfully, in this direction, I have no doubt. It has, however, other functions to fulfil. As regards one or two of these, I would say a few words. Their consideration influenced me, as I have no doubt it did others, in co-operating with my colleagues in the formation of the Society. Thirty odd years ago and after eight years of experience of medical work in the Tropics, during which I succeeded in learning this much, namely, that I knew nothing about tropical disease, I came home on furlough. After a month or two with my people, I came to London principally with the view to rub myself up in recent medicine and surgery, more especially in their bearing on tropical subjects. But it was like fishing in a big lake for the two or three fish the big lake might or might not hold. I did not know where the particular fish I wanted lay, and I found no one to tell me where they lay or how to set about hooking them. I finally landed at the Reading Room of the British Museum. Dreary enough and profitless enough was the fishing there, as you may imagine.

Nowadays things have improved vastly in this respect. There are post-graduate classes of all sorts; but I am not quite sure that a visitor from abroad with only a few months—it may be only a few days—to spare could put his hand at once on the special information he might be in search of, or on the person or persons who could or would guide him to that information. Nor—and this is an important consideration from the standpoint of a bashful man—would he feel quite certain that he was heartily welcomed by those who might be able to supply his wants·

Had such a Society as ours existed thirty-three years ago, I would not have had to go to the British Museum for my tropical pathology; I would have been put in touch at once with those who could have taught me, or been put in the way of being taught. Notwithstanding the greatly-increased facilities of recent times, there is still, in my opinion, room in London in the way I indicate, and especially in our particular branch of practice. The stranger might still have to search, perhaps in vain, for the hand that would welcome and assist him. I trust that our Society will supply this welcome and this assistance.

Again reverting to my personal experience, I once more came to London on furlough in the year 1882-3 with the same object in view—to learn the latest in medicine and surgery, especially in their application to tropical medicine. I heard plenty about the tubercle bacillus, but although I visited the Societies and became acquainted with many medical men of standing, I did not once hear of Laveran's important discovery of the malaria parasite. Indeed, it was not until I returned to China that I heard about it. I read Laveran's first book with great interest, and as I had abundant material at my command, I set to work to find the organism he described. Although repeated and prolonged attempts were made, I completely failed in my search, and almost became a sceptic about the existence of the plasmodium, and it was not until I returned to England in 1889, and not until I had renewed opportunities of working on the subject at the Seamen's Hospital, that I saw for the first time the malaria parasite. But by that time my chances of fruitful work had gone. I had no longer abundance of material at my command. My failure to find the parasite when in China was entirely attributable to faulty technique, not from want of opportunity. Had there been a Tropical Society in 1883, doubtless Laveran's discovery would have been a prominent

subject for discussion, the technique for its demonstration would have been familiar to the Fellows, and I should have gone back to China in a satisfactory position to pursue its study under favourable circumstances. I lost ten years by this.

Now, what happened to me in this matter of the malaria parasite, has, I know, happened to others equally interested in tropical medicine, equally keen to study it, and, if possible to advance it. And not only in this matter of the malaria parasite, but in other and similar organisms. They have had no opportunity of seeing these things, in their flying visits to this country, or of learning how to recognise them. They go abroad, and, although anxious to work, they are not in a position to work in a fruitful way. The tropical schools do much, but they do not adequately supply the wants of the flying visitor. I take it that our Society has a distinct *rôle* in this direction. It will be powerfully educative, and no one wishing to get abreast of the actual position of tropical medicine at the time of his visit to London need go away ignorant on any particular point, no matter how recently it may have cropped up.

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Dr. C. W. Daniels, Director London School of Tropical Medicine, gave an Epidiascope Demonstration of various microscopic and other objects of interest in Tropical Medicine.

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## MEETING HELD AT 20, HANOVER SQUARE, ON WEDNESDAY, JULY 10TH, 1907.

SIR PATRICK MANSON, K.C.M.G., President of the Society, in the Chair.

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DR. J. W. W. STEPHENS, of the Liverpool School of Tropical Medicine, showed specimens prepared by Captain S. R. Christophers, I.M.S., illustrating the Developmental Cycle of Piroplasma canis in the Tick :—

1. The parasite enlarged in the gut of the tick and became a motile club-shaped body, which then left the gut and penetrated an ovum, becoming in the substance of this a "zygote."

2. This zygote increased in size, and broke up into "sporoblasts," which were found disseminated in the tissues of the larva.

3. These sporoblasts further divided and accumulated as "sporozoits" in the salivary glands of the nymph.

4. Sporozoits accumulated in the salivary glands of the adult tick. Experiment showed that the adult took infection and the nymph and "daughter" adult gave infection, and, corresponding to this, "sporozoits" were found in the salivary glands of the tick at both these stages. The cycle of development appeared thus to be a very simple and uncomplicated one.

The next exhibit shown by Dr. Stephens was one of the parasite discovered by Darling in Panama, *Histoplasma capsulata* (1906). The parasite was an intracellular one resembling somewhat Leishman-Donovan bodies, and existed in immense numbers in the lungs, the liver, and the spleen.

The following new helminths were also exhibited: 1. A new Bothriocephalid in man from Tasmania or Syria; 2. A new Nematode in man from China; and 3. A new Echinorhynchus in man from British Honduras.

# THE TREATMENT OF TRYPANOSOMIASIS.

By Professor B. MOORE and Drs. M. NIERENSTEIN and T. L. TODD, of the Liverpool School of Tropical Medicine.

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(*Read by Dr. M. NIERENSTEIN, Wednesday, July 10th*, 1907.)

The therapeutics of the present time for trypanosomiasis can be divided into the following :—

A. *Arsenic Compounds.*—1. Arsenious acid ; 2. Fowler's solution.

B. *Colours.*—1. Colouring matters belonging to the diazo group, Ehrlich's trypanred and Mesnil's afrodol blue and afrodol violet ; 2. Colouring matters belonging to the triphenyl-methan groups, Ehrlich's malachite green and fuchsin.

C. *Atoxyl,* which being an organic compound of aniline and arsenious acid cannot be considered as merely "an arsenic compound," an expression recently used in the lay Press.

The introduction of an acid radicle into an amido nucleus generally changes its basic character, and, of course, on the other hand, lessens the acidity. The combined effect of aniline and arsenic is, therefore, the resultant of these two factors: amido group and arsenious acid. We have been able to demonstrate experimentally that either the amount of aniline generally given in the form of atoxyl, or the arsenious acid contained in that drug, when administered alone, will kill an animal within a few hours after the injection.

D. *Combined Treatment.*—1. Combination of arsenious or acid atoxyl with colouring matters, as trypanred, suggested by Thomas and Breinl, Laveran and Mesnil, and

others ; 2. Atoxyl and strychnine — van Campenhout ; 3. Atoxyl and sublimate. The last-named treatment has recently been worked out in the Liverpool School of Tropical Medicine, and the results obtained on rats are very promising.

We who have been working in the Liverpool School of Tropical Medicine found that in infected rats (*Trypanosoma brucei*), one part of which were treated with atoxyl alone, and the remainder with atoxyl followed by mercury, the first lot generally had relapses in from sixeeen to twenty-five days, while a great number of the animals treated with atoxyl and mercury are still alive, some after a peirod of nine months. These results are certainly encouraging. In our treatment we used at the beginning to give high doses of atoxyl in order to drive out the parasites entirely, and it was found that if sufficient atoxyl were administered the parasites (*Trypanosoma brucei*) would disappear in rats for from sixteen to twenty-five days. A disappearance of the parasites for a similar period was also observed in guinea-pigs and rabbits. Mercury was then given, and the treatment stopped. In the bigger animals this double treatment should be continued for some time.

With regard to sleeping sickness, we would like to suggest, as a routine treatment, that one cubic centimetre of a 20 per cent. solution of atoxyl should be given daily for one week, followed by one cubic centimetre of a 1 per cent. solution of sublimate four times. This treatment should be repeated and continued for an extended period, and, of course, may require occasional modification. It would perhaps be possible to give mercury in the form of pills. The theory of the action of mercury in this treatment has been discussed in a paper published in the *Annals of Tropical Medicine and Parasitology, of the Liverpool School of Tropical Medicine*, vol. i, No. 2.

It has been found that the toxic effect of atoxyl can be

diminished by acetylating the atoxyl. Such an acetylated atoxyl is an antipyrine which contains arsenious acid. Acetylated atoxyl has been shown to be of use in mice by Ehrlich, and in dogs by our experiments. We found, as a rule, that when atoxyl was given in sufficient quantity to drive out the parasites (*nagana*) in dogs the host was also killed, but using acetylated atoxyl we have been more or less successful. An interesting fact pointed out by Ehrlich is that the parasites can get used to a drug and become resistant. It is therefore advisable to alternate various trypanocidal agents in the treatment as much as possible, that is, to give atoxyl, some other trypanosome-killing drug, and mercury; for instance, atoxyl, fuchsin, or some other colouring matter by the mouth, and sublimate.

It has usually been recommended to give atoxyl in high doses at the beginning. Whether or not this is of benefit is a question, as the atoxyl apparently accumulates in the body, and the toxic effect appears after a time. In laboratory experiments we have frequently noticed that even after the treatment had been stopped for a week toxic effects appeared, and this could only be due to the fact that atoxyl had accumulated in the body, and the arsenic had been slowly broken up. We would like to refer to one experiment. A rat was treated about nine months ago with atoxyl and is still showing toxic effects.

We think it necessary to point out that the administration of atoxyl should be given a very careful trial, but that extremely high doses should be avoided as much as possible. The action of atoxyl seems to be very much on the nervous system, as neuritis is frequently caused by the administration of this drug. Blindness has also been observed after the use of atoxyl, and the question therefore arises whether it would not be well to counteract somewhat the effect of atoxyl by making it less operative on the brain and nerves. This might be done by introducing radicles

which are more fixed to the blood and less to the nervous system.

We think it necessary to draw attention to experiments which we have made in connection with the reappearance of trypanosomes after treatment with atoxyl. We found that strains obtained from animals which had had relapses were sometimes much more virulent. Our *nagana* strain, used for work generally, killed a rat in from five to seven days. This strain was followed up, commencing on January 14th until May 5th, by subinoculation from one rat to another. Four drops of blood were always used. In none of these rats was any sudden increase of virulence noticed; but in experiments with the strain obtained from animals in which the trypanosomes had reappeared it was found that the experimental animal was killed in from three to five days. Out of seventeen rats used for these experiments, eleven showed a remarkable increase of virulence. Similar work done with *Trypanosoma dimorphon* showed the same increase of virulence. Our strain used to kill a rat in from fifteen to seventeen days, and we found that in four out of six strains obtained through relapses after treatment by atoxyl the virulence obtained was from eleven to nine days. We have recently obtained through atoxyl a *Trypanosoma dimorphon* strain which kills a rat in seven days. Similar work with *Trypanosoma gambiense* is now in progress.

It was our intention not to publish the results of this work until we had satisfied ourselves that *Trypanosoma gambiense* behaves in a similar way, but since it is intended to commence atoxyl treatment on a large scale in Uganda, we feel that a warning in this direction should be given.

These observations emphasise the necessity for careful selection of "fly-free" sites for the separation camps and for some provision by which treated natives may be kept under observation for considerable periods.

The atoxyl and mercury treatment lately used in our laboratories gives, when compared with atoxyl, fairly permanent results, and if an atoxyl treatment should be undertaken in Uganda, it would be advisable to start at once with this combined treatment, even if the reliability of this double treatment has so far not been fully established.

Summarising, we would like to suggest, in the treatment of sleeping sickness, a fresh 20 per cent. solution of atoxyl, warmed up to 40 deg., to be administered in small doses to commence with, and the doses to be gradually increased, not, if possible, passing the limit of one cubic centimetre of a 20 per cent. solution. The atoxyl to be followed as soon as possible by mercury in the form of sublimate, and, in addition, some other trypanocide to be given. We would especially recommend fuchsin. We have lately tried a large number of colouring matters, both Ehrlich's and Mesnil and Nicolle's, and have come to the conclusion that fuchsin is the most promising. In addition to this, as was insisted upon by Thomas and Breinl, the general strength of the patient must be sustained in every way possible.

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Dr. Anton Breinl, Director of the Runcorn Research Laboratories, described and demonstrated the morphology of *Trypanosoma gambiense*, and the changes which it exhibits: 1. During reproduction; 2. During treatment of its host by atoxyl. The effect of the latter drug, he said, was to cause the parasite to secrete a capsule, and to then withdraw into the internal circulation. In the blood-vessels, reproduction by sexual conjugation did not take place; at least it was never found, but division, after distinctive changes in nucleus and blepharoplast, was very common. He further demonstrated a new method of making blood films by first spreading a layer of liquid albumin on the

slide, which prevented shrinkage of the protozoal tissues when spirit was applied to stain or fix the specimens. By Flemming's solution, and the subsequent use of safranin, polychrome methylene blue, and orange tannin, details impossible to be seen by the older methods could be well made out.

Dr. Todd (Liverpool School of Tropical Medicine) read a paper "On Some Protozoal Parasites found in Animals on the Congo." He described the phases seen in a leucocytozoon from the blood of a hawk, which had been described by Schaudinn, and of which male and female forms were seen. The host (a grey hawk, *Asturinula monogrammica*, common on the Congo), harboured large numbers of parasites, but it was probable the name Leucocytozoon would have to be changed, as *L. Ziemanni* occurred in other than white cells, and it was not related to the Leucocytozoon of mammals, which was a true white-cell parasite. Prior to conjugation, the matured parasite became rounded, and the sheath of periplast was shed, with the effete nucleus of the leucocyte post-cell. There were resting and motile stages, as noted by Schaudinn. Nuclear material was abundant in the male, less so in the female. This chromatophilic structure passed through several definite stages in development, each of which had distinctive appearances, and these Dr. Todd described. The whole process was akin to the trypanosome stage of *Halteridium noctuæ* (Schaudinn).

Dr. Todd said that among other protozoal parasites of amphibia, *Trypanosoma loricatum* was found in several species of frogs and toads. It was seen only in the blood, in which Drepanidium was almost always present, but there was no definite relation as to numbers. The division of *T. loricatum* was peculiar, and various forms, from Herpetomonas-like bodies to the original typical trypanosomes, were to be seen; the whole process was continuous,

but full of interesting stages, *e.g.*, rotation in opposite directions of dividing cells, division into sixteen cells within the envelope of the trypanosome, and subsequent development of a flagellum.

Dr. Todd said that present methods of staining were insufficient for protozoal work ; they showed the structures but no detail. The study of protozoa was widely separated from that of bacteria, and must be approached from a different basis, and with the knowledge which we already had that the life-cycles of many species were highly complicated. Continuous observation of living parasites was essential, and parallel hypotheses, acquired by knowledge of malaria, were only useful as aids to interpretation.

Sir Patrick Manson said the Society was greatly indebted to the members of the Liverpool School, who had, at considerable inconvenience to themselves, travelled to London to give them the excellent demonstrations and observations which they had listened to that evening. The development of piroplasma was of great interest and importance, and the researches of Captain Christophers would, he believed, throw much light on a problem in protozoology of which little had hitherto been known. At the request of Dr. Todd, Sir Patrick Manson described the results he had experienced in the treatment of Europeans affected with trypanosomiasis by atoxyl. Several cases under his care undergoing this treatment were doing well, and he founded great hopes on the method. Just as mercury was given in syphilis, so should atoxyl be administered in trypanosomiasis, namely, in small doses for prolonged periods. One case so treated was now free from fever, the patient's blood did not infect when inoculated into animals, and he was apparently cured. He regarded the clinical question as being in many ways parallel to that of syphilis, and said that with care, abundant nourishment, and treatment in the earlier stages, there was a moderately

hopeful prospect of ultimately controlling the disease, or, at least, materially reducing the mortality. He further described at length the cases of other Europeans, and said that he agreed that atoxyl followed by mercury was a rational, and possibly would be found to be a successful, treatment.

# A FEW NOTES ON SLEEPING SICKNESS.

By SIR HARRY JOHNSTON, K.C.M.G.

(*Friday, October* 18, 1907.)

What I am about to say is probably well known to all who have studied the subject who may be present to-night. But it might be useful to recall to the meeting some of the old and the new facts about sleeping sickness.

It is fairly certain from the Arab records of Western Nigeria that some form of sleeping sickness was known there about the twelfth century of the present era, and that several notabilities died of this sleeping disease. It was certainly heard of in sporadic cases on the Gambia, at Sierra Leone, and in the western part of Liberia between about 1785 and 1840. Between 1820 and 1870 it occurred with some frequency in the coast region of Liberia. It is not by any means extinct in that country yet. The Mandingo trader, whose portrait appears as a frontispiece to my book on Liberia, and who travelled with me about that country in 1904, died of sleeping sickness at the end of 1905.

Winwood Reade alludes to the disease as one that is well known on the West Coast of Africa. This would be about the middle of the 'sixties of the last century. I heard a good deal about the sleeping sickness when I first visited the Congo in 1882-1883, but the disease apparently did not exist then (or was not mentioned to me) east of Stanley Pool. I only heard of cases on the north and south banks of the Congo between Matadi and the sea.

The opening up of the Congo by Stanley's expedition and other agencies seems to have carried sleeping sickness

on to the upper reaches of that river, from which it rapidly spread to all parts of the Congo basin, making a special nidus in the Ituri Forest on the north-east and on the Upper Aruwimi (I quote from information given me in 1900 by Swahili traders and intelligent natives coming from those regions). Emin Pasha's Sudanese, when they settled down in the Lendu country to the west of Lake Albert, seem to have become infected with the disease. A portion of these troops were moved somewhat rapidly into Busoga, the district of the Uganda Protectorate which is on the opposite bank of the Victoria Nile to the Kingdom of Uganda. After the Sudanese troops of Lugard's recruiting had thoroughly settled down in Busoga, sleeping sickness began very slowly to develop. Possibly its spread was checked by the convulsions and displacement of population occurring during the Uganda Mutiny. I first heard of cases of sleeping sickness which reminded me of those I had met with many years before on the Congo in the early spring of 1901, when I was visiting the coast of Busoga and the island of Buvuma. Since that the history of this disease in the Uganda Protectorate is too well known to require recapitulation.

But what deserves special attention at this moment is the appalling ravages of sleeping sickness in the western part of the Congo basin, as reported by travellers like Torday and the numerous missionaries. It seems to be killing out a number of the Bayaka on the Kwango River. In some of these districts it would almost seem as though sleeping sickness had returned; would seem indeed as though somewhere in the Congo basin this disease had acquired a sudden and very serious virulence. I cannot recall any traditions or recorded history of the West Coast of Africa, from the Senegal to the Congo, in which anything like a serious epidemic of sleeping sickness is mentioned. Through the centuries which have elapsed since

the Arabised negroes inscribed the history of the Muhammadan Mandingo kingdoms round the sources of the Niger, sleeping sickness has been a known disease, occurring sporadically here and there, but never to such an extent as to create widespread alarm or serious depopulation. It scarcely bore this devastating character even in the early 'eighties, when it affected the Lower Congo. Indeed, the records of the Baptist Mission at San Salvador, etc., would seem to show that sleeping sickness has come back there from the east, from the inland basin of the Congo, and is afflicting the country far worse than it did in the 'sixties and 'seventies. The matter is really becoming very urgent for those who take an interest in the commercial development of Africa, as the spread of the disease is attaining such proportions as may almost end in the depopulation of the Congo basin, to say nothing of the unchecked ravages in the Uganda Protectorate. Its range certainly seems to be limited by the range of one or more species of tsetse fly that do not care about parts of Africa without heavy rainfall and abundant vegetation.

# ON SLEEPING SICKNESS AS MET WITH IN UGANDA, ESPECIALLY WITH REGARD TO ITS TREATMENT.

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(*Friday, October* 18, 1907.)

Like many other tropical diseases, sleeping sickness has come into prominence lately, partly owing to the increased study devoted to all tropical ailments, and partly owing to the importance it has assumed and the economic interests involved by its attacking a flourishing and important British colony. In the last seven years some 60,000 natives have died from this cause in Uganda, and their deaths involve a double loss to the Government. In the first place they were for the most part men in the prime of life, paying hut-tax, etc., and thus directly profitable to the Administration, as well as forming no inconsiderable part of the wage-earning members of the community, and their deaths also directly decrease the by no means large population. These considerations, important as they are, are not, however, so serious as the well-grounded fear that the disease will go on spreading, and involve first the surrounding countries, and afterwards whatever parts of Africa are inhabited by the tsetse fly.

Here let me diverge a moment to note the interesting development of the study of tropical medicine. When I left for Central Africa in the autumn of 1896, but little interest was taken in tropical diseases by the profession as a whole; the fascinating story of the evolution of the malarial parasite was unknown, and the intense importance

of a knowledge of zoology—at least in so far as it concerned the characteristics of the animal parasites of man and the domestic animals—not fully grasped. Haematology was only in its infancy—Davidson's *Tropical Diseases* almost the sole book available to the student. But in the last ten years what a change—*tempora mutantur et nos mutamur in illis*—a remarkable output of tropical literature issues from the Press, several excellent journals devote themselves to the subject, and there is no lack of books on tropical medicine. The London and Liverpool schools are known all over the world for the sound training they give their scholars, and the coping-stones have perhaps been added by the special diplomas in tropical medicine granted by Cambridge and Liverpool.

The formation of the Society I have the honour of addressing will doubtless bring together those most distinguished in this department of medicine. Even the attitude of our patients is changed. A medical man is hardly considered qualified unless equipped with his oil-immersion lens and hæmatological apparatus, and the sufferer expects films to be taken from his blood as much as he expects the thermometer to be placed under his tongue or the stethoscope to be applied to his chest.

It is no wonder then that the subject of sleeping sickness, presenting as it does so many interesting problems for research in its parasitology, ætiology, etc., and in the manner of its spread, should have a very large bibliography, the great mass of which has been produced in the last five years. Owing to the wideness of the subject, I shall chiefly confine myself to-night to a consideration of the means we have at our disposal for *treating* this fell disease.

My own acquaintance with it began towards the close of 1900, when several suspicious cases fell into the hands of my brother and myself. Believing at the time that Manson's theory of the disease, being in some way causally

connected with *Filaria perstans*, was a correct one, we searched for and found these parasites in every case, a fact which confirmed our suspicions, as although we had made hundreds of blood examinations before, we had but seldom met with the parasites. Although the careful researches of Dr. Low and others have amply demonstrated that this is not the case, and that sleeping sickness attacks people who have no *Filaria perstans* in their blood and conversely *Filaria perstans* is met with freely in regions where sleeping sickness is unknown, yet in areas common to both, sleeping sickness seems to favour the presence of *Filaria perstans*, the latter organism being found very frequently in sleeping sickness cases.

The rapid spread of the disease gave considerable anxiety to the Government, and from 1902 onwards successive Royal Commissions have vigorously worked at the subject. By the help of the Government medical officers and others the whole country of Uganda has been made the subject of a prolonged and exhaustive enquiry, and much light has been thrown on this strange and interesting disease.

The first big step forward was taken when Colonel Bruce pointed out the causal relationship to this disease of the trypanosomes discovered in the cerebro-spinal fluid of sleeping sickness cases by Castellani.

The ink was hardly dry on Castellani's thesis attempting to prove that a certain diplococcus discovered by him was the cause of the disease when this startling announcement was made, and Colonel Bruce following up the chain of reasoning he employed when investigating the trypanosome of Nagana in South Africa, and proving its connection with *Glossina morsitans*, demonstrated in a most brilliant and convincing manner that the endemic centres and areas of sleeping sickness coincided with those for a species of the tsetse fly known as *Glossina palpalis*, and that the spread of the disease only took place in areas inhabited by the

tsetse fly. Subsequent research has only confirmed his first hypothesis.

The morphology of the trypanosome has been pretty thoroughly worked out, also its method of propagation, by Greig, Gray, and others, while the morbid anatomy of the disease, both macroscopic and microscopic, is familiar through the descriptions of Mott. Professor Minchin has exhaustively studied the anatomy of the tsetse fly itself, though some features in its life-history still remain to be cleared up. The different Reports issued by the successive Royal Society Commissions are a mine of information on these and kindred points.

The general clinical history of a case is now well known to the profession, both from papers contributed to tropical periodicals, and also by the very careful description given by Manson, first of the native cases imported from the Congo, and finally of several Europeans who were treated in England.

In a suspected case of sleeping sickness, centrifugalisation and examination of the blood give but little help, for they require too long a time for the busy practitioner; even Koch, with his large laboratory staff, has given up this method as being unsatisfactory; but centrifugalisation of the cerebro-spinal fluid obtained by lumbar puncture is more satisfactory, the sediment frequently showing motile trypanosomes when examined with a $\frac{1}{6}$th or $\frac{1}{8}$th objective. Captain Greig has, however, given us by far the best general method for clinical use, *i.e.*, by puncture of the swollen cervical glands, so frequently met with in this disease. The resulting gland juice when examined under a $\frac{1}{8}$th objective will usually show trypanosomes, and these are frequently numerous, even in early stages of the disease. It is only fair to add, however, that even in well-established cases we have not always detected trypanosomes even on repeated puncture, that in some people with

small glands, especially if the latter are hard and shotty, the amount of gland juice obtained may be very small, and it may indeed be very difficult to obtain any, and that only the positive recognition of the trypanosome is valuable, since enlarged cervical glands are common in syphilis (from which 10 to 15 per cent. of the population suffer), from impetigenous diseases of the scalp and face, ulcerated throats, etc.

The natives themselves are often more accurate than we are in the detection of quite early cases, some subtle mental change making them suspicious even before any glandular enlargement can be detected.

The extreme frequency of an excited mental state, often amounting to positive mania, has hardly been laid enough stress on by descriptive writers. Pyromania is a very usual form, and considerable damage has been done by persons afflicted with this disease. Contrary to what one might expect, this is often manifested in quite an early stage of the malady. Religious mania is another common form, and homicide is occasionally met with. The value of rashes, œdema, etc., on the bodies of the dirt-begrimed native, though doubtless these are often present, is of comparatively slight value. An irregular temperature, with a quickened pulse, are useful in coming to a diagnosis.

The question of treatment has been less worked at than other features of this disease. Out of eighteen pages devoted to sleeping sickness in an admirable article in the volume of tropical diseases just published in Allbutt's *System of Medicine*, only nineteen lines are given under this heading, so that one has ample excuse in laying such facts as have come under one's personal observation before you.

First, to deal with the question of prophylaxis. This is of extreme importance to Great Britain as regards her African Colonies and Protectorates. For a long time, nearly

a century, in fact, after the existence of this disease had been made known, viz., by Winterbottom in 1803, it was known only to exist in the Congo and certain West African States, or in natives deported from these regions. The progress of civilisation, however, with the increased facilities of travel, have very materially altered the conditions existing even as late as the end of the nineteenth century. In 1896 the caravan in which we travelled took no less than three months to traverse the space of 800 miles between Mombasa and Mengo, the capital of Uganda. The same distance is now accomplished by train and steamer in three and a-half days. This is only typical of the rest of Africa. West is now being brought into contact with east, and north with south, in a way quite impossible a few years back; and *pari passu* with this, diseases which were confined to one area are spreading along the lines of communication to other areas where favourable conditions obtain. For example, the jigger (*Sarcopsylla penetrans*) first made its appearance in Uganda from the West Coast in 1892; in 1896 we encountered it at Machakos half-way to the East Coast; a few years later it reached Mombasa on the coast, and is now found over much of the continent. The first case of spirillum or relapsing fever was noted in our hospital registers in 1899. It has now become so frequent through the spread of infected ticks that we have at least five times as many admissions from this cause into one hospital as from malarial fever. So with sleeping sickness. I do not believe it was prevalent to any large extent before the commencement of 1899 in Uganda. Some authorities put it as early as 1892, but my duties as a medical missionary cause me to travel considerable distances in Central Africa, and wherever I go the sick come for treatment, sometimes to the extent of 400 or 500 in the day. In this way I have been from the Congo State on the west to Mount Elgon on the east, and from the islands of the Lake

on the south to the Sudan on the north. Our hospital, too, by far the largest in Uganda, draws its patients from a very wide area, so that it is very improbable that the disease existed to any great extent long anterior to 1900, at the close of which year we admitted our first in-patients suffering from it. I emphasise the point because I wish to lay stress on the extreme rapidity with which the disease spread. In 1900 the first case was notified, and by the middle of 1902 it was deemed to be so serious that a Commission of the Royal Society was engaged in investigating it. Accounts of the long incubation period of the disease have rather tended to hide the fact that it can, and does, spread with great rapidity.

As regards the present condition of affairs in Uganda, the outlook is a very gloomy one so far as it concerns the possibility of hindering the spread of the disease to non-infected glossina areas. At an early period, indeed, Dr. Moffat, the then Protectorate Medical Officer of Uganda, asked for £4000 to segregate those suffering from the disease. The Government were unable to afford this, and nothing was done. It is a significant fact, however, that £4000 was spent in a single year lately in cutting down brushwood and undergrowth round one Lake station alone of the Government.

The discovery of Colonel Bruce as to the spread of the disease by infected glossina flies simplified matters, and gave a ray of hope, for could those suffering from the disease be prevented from travelling to non-infected areas, the spread of the disease could be checked. It is interesting, however, to notice the extreme complexity of the factors in this case. It has been the custom for the natives who were unwilling or unable to pay their hut-tax to put in a month's labour for the Government in building, clearing land, etc. Much of this needed to be done at Entebbe, a place on the Lake shore, the administrative

capital of the country. The natives in or near the larger towns and villages were more easily able to obtain by trading or selling their produce the hut-tax of three Rs., so that it was rather the poorer class of native that came in, often from the more remote parts of the country, to labour on Government works. These men were housed in grass huts run up by themselves on a small area of land close to Entebbe for facility of getting at their work, only a short distance from the lake, whence they drew their water, and near a bit of forest land, which provided their firewood. It was not for some time that the full bearing of Colonel Bruce's discovery that the disease was carried by *Glossina palpalis* on the spread of sleeping sickness was realised. Meanwhile, for some years, these crowds of labourers were living on what was afterwards ascertained to be a district beloved of the tsetse fly. At the end of a month they would return to their homes, many doubtless infected with trypanosomes, but as yet showing no obvious symptom of the disease, and a fresh generation of workmen would take their place to be in turn affected. Meanwhile, since *Glossina palpalis* is very widely distributed along the swamps and watercourses of Uganda, the flies in hitherto untouched regions drew a supply of trypanosomes from the blood of these returned hut-tax labourers, and in their turn infected those natives who had not left their own homes.

Owing to the relatively long incubation period of the disease, these ambulatory cases are of course numerous, and conversant as we are with a large area of the country, the sequence of events is distressingly familiar. A single case is reported from a village hitherto spared. Usually this will be in a man who has either returned from Entebbe or who has been sojourning, at any rate for a time, in an infected area. For some weeks or months nothing more is heard, and then enquiries show that several show signs of the disease. Some of the natives then abandon the village,

a certain percentage no doubt carrying in themselves the germs of the disease, but the majority with the fatalistic habits of the native, remaining only to die in their homes.

This goes on till the village is exterminated. It is most melancholy visiting the shores or islands of the Lake. Where six or seven years ago was a contented and relatively dense population, to-day reigns desolation and sadness, the empty and fallen huts being rapidly lost in the overgrowth of tropical vegetation.

The disease has now far outstripped Uganda proper. Northward it has spread down the Nile to the south borders of the Sudan at Gondokoro, westward it has encircled the Albert Lake, eastward the large and populous country of Usoga has been decimated, and it has carried off many in Kavirondo; while southward, German East Africa has already been attacked. But worse seems likely to follow. There is a belt of tsetse flies between Uganda and the coast, and should this be infected a very serious spread of the disease seems likely. The latest news is that cases are occurring in Portuguese East Africa, whence there is reason to fear its making its way into Rhodesia. Indeed, no district where *Glossina* is found can be said to be safe from the ravages of sleeping sickness. At present it is not yet definitely proved that other species of *Glossina* can act as carriers of the trypanosomes, but the balance of opinion seems to be that this is possible even if not probable. Were this the case, the problem becomes still more complex. The Germans are so impressed with the danger that they have equipped and sent out at great expense a Sleeping Sickness Commission, which has planted itself on one of the worst-infected islands, and put their greatest bacteriologist at its head.

What is being actually done now in Uganda? Four important methods are being adopted. First, those actually suffering from sleeping sickness are being collected as far

as possible, and segregated in isolation camps, where each man, the presence of trypanosomes having been verified in his body, undergoes a thorough atoxyl treatment.

The first camp to be established was at a place called Buwanuka. Some two hundred were collected there when, in February last, at the kind invitation of Dr. Wiggins, I made a thorough inspection of the place. Each case was thoroughly treated with atoxyl, full details of the injections given with the length of the stay, presence or absence of trypanosomes in the gland juice, district from which the patient came, each being noted and tabulated. Every care was taken to remove prejudices from the native mind, and an excellent work was being done.

A new camp has been recently opened at a place called Kyetume, in Kyagwe, and a third is projected near Iganga, in Usoga.

The second method is the compulsory eviction or removal of all natives from infected tsetse-fly areas. This, alas! bristles with difficulties. The Uganda police are too few in number to efficiently patrol a large area. The natives cling to their old homes and plantations, their fatalistic tendencies breed a resigned attitude to the disease, and not one in a thousand believes that the tsetse fly, which they call "Kivu," really conveys the disease. Finally, if evicted, they will often steal back to die in their own homes. Moreover, the islands, the worst foci of the disease, are almost necessarily left alone.

The third method is the clearing extensive areas round landing-places, markets on the Lake shore, watering-places, and the necessary Government or trading stations on the Lake.

The fourth method is the printing in Luganda, the native tongue, clear instructions as to the nature of the disease, how it is spread, the importance of clearing away the undergrowth in which the "Kivu" lives, and the urgent

need for segregation of those actually sick, while they enlist, by every means in their power the active co-operation of the chiefs. These chiefs are by no means fools. At a gathering of them convened for the purpose, I was asked by Colonel Bruce to interpret for him, as he endeavoured to explain to them how infection was spread by the tsetse fly. Respect for the Europeans kept them mostly silent; but one pertinent question they asked: "If," said they, "the disease is transmitted from a sick person to a healthy by the bite of a 'Kivu' fly, which injects the germ from the one to the other, why are fleas, bugs, and other biting insects and fleas incapable of conveying the disease in like manner?" To anyone knowing Uganda, the extraordinary number of biting pests may well explain this question, and indeed it is quite possible that stomoxys may act as a mechanical carrier of the disease if the sick live in close contact with the healthy.

The credit for this organised effort to cope with the disease belongs largely to Dr. Hodges, the present Government S.M.O. in Uganda.

As regards personal prophylaxis, the European, even living within the dangerous area, is but little exposed to infection if he uses his knowledge intelligently. His bungalow should not be built near the water's edge, nor close to native compounds. The ground for a considerable area round his station should be cleared, and what is more difficult, kept clear of undergrowth, especially of bushes, in the shade of which the tsetse fly loves to disport itself. His dwelling should be protected by gauze screens or fine wire netting, and his bed by a mosquito net; while the native porters who bring his water from the Lake should not be allowed to enter the house, for tsetse flies often buzz persistently round the head and shoulders of these porters, and follow them for long distances.

Tsetse flies have been discovered *inside* European bath-rooms at Entebbe, at some considerable distance from the Lake, doubtless introduced in this way.

Professor Koch, in a conversation with me on the subject, told me that he and the officers of his expedition were simply depending on living away from the water's edge, which the tsetse fly frequented—their camp is some 400 ft. above Lake level—and on wearing white garments, since the fly was repelled by white but attracted by dark clothing.

To deal finally with the means to be employed when a patient actually attacked by the disease comes under our treatment. Let me begin by deprecating the premature and highly-coloured reports of alleged cures reported in the daily Press. Again and again have we seen it stated or hinted that this method or that has been successful. Koch, who has been as much maligned as anyone in this respect, himself told me that he had no expectation of curing patients already stricken down. His hopes lay in the direction of prophylaxis. The early medicines employed had no effect; quinine in large doses, the ordinary arsenical compounds, etc., proved useless. In our mission hospital, which contains some 120 beds, we have made a point of testing carefully and accurately various remedies, for, situated close to the fringe of the sleeping sickness, we are never without sleeping-sickness cases which can easily be multiplied to any desired number, when we wish to try the effect of a particular remedy.

The good effects noted to be produced on cattle affected by trypanosomiasis in West Africa by hypodermic injections of arsenic led me to give this method a careful and extended trial in 1904 and 1905. Though hopeful at first, the good results were seen to be temporary only, and the method was finally abandoned. A 1 per cent. solution of sod. arsenite was used, and hypodermic injections were

made, after sterilising skin and needle carefully, in the buttocks and inter-scapular regions twice weekly.

An initial dose of 5 minims was given, which was raised by 5 or 10 minims each week till slight symptoms of arsenical intoxication were produced. These set in when from 1 grain to 1½ grains of the salt were administered at a time, the quantity depending on the general state of health of the patient, and, what was perhaps the same thing, the stage of his disease. This method had grave drawbacks—the injections were undoubtedly painful, though in only one or two cases out of a great many injections did an abscess result, and when arsenical intoxication set in, it was difficult to control the resulting symptoms. One man who was months under treatment did very well till the amount of arsenic injected reached 1½ grains, when a copious and distressing rash broke out, hard to treat, and covering the greater part of his body in extent, and the misery of which caused him to refuse further treatment and to leave the hospital.

Microscopic examination of the gland juice showed that after the patient had been treated for some time with arsenic the trypanosomes decreased in number very greatly, even in bad cases, and disappeared entirely in certain cases. One great difficulty in this as in every other form of treatment was in persuading the native to stay in the hospital long enough to undergo a thorough trial with the drug, more especially the slight cases, in which presumably most good could be done. If put on the out-patient list they could not be trusted to come up at regular times.

The need of caution in claiming a cure hastily, as well as an undesigned control experiment, was impressed on my mind by a case with whom we have been in touch for the last two years. There were two native young men, of much the same age—about nineteen—both thoroughly healthy, who went to Entebbe to work in the Government printing-

office. After a residence of some two years it was found that both had trypanosomes in their gland juice. They were both asked to come for treatment into our Mission Hospital. The one, named Timoteo, would only consent if we would guarantee to cure him. This, of course, I was unable to do. The other, named Simeoni, came in and underwent a three-months' course of arsenical injections. He was then lost sight of for twelve months, at the expiration of which time he turned up looking as fat as butter, the picture of health, and just about to marry! The only symptom about him of the disease were a few tiny glands in his neck. Puncture of these showed no trypanosomes in the gland juice. Here, then, was a case that looked like a cure; but I was sceptical. Putting him under chloroform, I dissected out one of the deeper glands, and after a long hunt succeeded in finding a single trypanosome. He has since undergone a careful atoxyl treatment, and I trust is doing so still. The control case, who had no arsenical treatment, went too late to Professor Koch, and died at his camp just about the same time.

We did not try trypanroth, as it is expensive and hard to get, and thus not suited for experiments on a large scale. The Royal Society Commissioners who used it at Entebbe reported it was inefficient.

*Atoxyl.*—This is by far the best drug that has yet been tried, and it is greatly to the credit of our English investigators, who have all along stood in the front rank of the army of successful research in tropical medicine, that an Englishman—Thomas of Liverpool—first applied it to the treatment of sleeping sickness.

A brief but good account of the drug is given in the last edition of the *Extra Pharmacopœia*, where it is called meta-arsenic-anilide, containing 37 to 67 per cent. of arsenic; but Dr. Levy Bing, as quoted in the *British Medical Journal* of August 3 last, states that it is not the anilide

of meta-arsenic acid, but a monosodic salt of the anilide of ortho-arsenic acid, and was discovered by the French chemist Bechamp as far back as 1863, and only contains 29 per cent. of arsenic. He obtained it by heating arseniate of aniline.

Atoxyl is a snow-white powder, fairly soluble in water, the latter taking up about $\frac{1}{8}$th part by weight of the salt. A fact which Martindale does not mention is that it slowly decomposes, especially if exposed to sunlight, the solution turning bright yellow, and presumably aniline being formed.

A saturated solution of this salt in distilled water forms a 20 per cent. solution. Of this we use 2 c.c. as a dose. At first we followed Koch's method, and gave a hypodermic injection with the usual aseptic precautions and in the usual gluteal and interscapular regions on each of two successive days. An interval of eighteen days was then allowed to elapse and two more injections were given. The patients were then discharged and allowed to come up as out-patients. Lately we have greatly increased the frequency of the doses, though not the strength of individual doses, giving for example a dose on every other day, or in one case on every day for seven or eight times, and repeating the doses on two successive days every fortnight. Atoxyl has certain obvious advantages over sod. arsenite. The injection is certainly less painful, and the results are more certain and more speedy.

The advice given to warm the solution before injecting is not needed in the tropics. We have only had one abscess —in a cachectic subject—out of many injections, and the patients object very little to the treatment.

The immediate effect on a bad case is really magical. To take two cases, one a boy named Bukuku. We had watched him in every stage of the disease (for it had first appeared when he was an in-patient suffering from an attack

of gonorrhœal rheumatism), from its inception till from being a clean, intelligent boy he had become completely bed-ridden, passing his water under him for several weeks, and merely uttering gibberish or vacantly staring when addressed. Atoxyl was then tried, and though at first little good effects were seen, he steadily—though in this case only slowly—improved. He rose from his bed, became clean again in his habits, and was able to walk and even to run, so far as his joint-trouble let him, while his mental condition improved so much that he was able to answer intelligently questions asked him. Yet this very case illustrates the defects and limitations of the treatment, for only by last mail I heard of his death.

Even when cases apply for treatment in too late a stage of the disease for any hope of cure by atoxyl, yet it seems to powerfully influence the nutrition of the body, for they die without bedsores or extreme emaciation—the stage of dying is, so to speak, rendered more comfortable for them.

Microscopically, the trypanosomes, after a full dose of atoxyl, can no longer be found in the gland juice a few hours after the first injection. There must be a great destruction of them somewhere, and yet others, perhaps immature or encysted forms, must survive, for the fact is being slowly recognised that even after repeated and full doses of atoxyl the disease is not cured—sooner or later the trypanosomes reappear.

As regards dosage, we are still feeling our way. I have lately been employing considerably larger amounts than I understand Professor Koch uses, giving in young adult cases a quantity equivalent to 6 grains of the solid salt every other day, or even every day, for six or eight injections. Signs of arsenical poisoning are certainly unusual, and digestion is but little interfered with. Headache is occasionally complained of, but this is not uncommon in the disease itself. A medical friend of mine who had been

employing atoxyl, told me he thought he had seen four deaths in the cases under his charge from over-dosage, but the evidence he submitted did not satisfy me that the medicine was the cause of the deaths.

Just before leaving Uganda, I commenced giving mercurial salts with the atoxyl, following a suggestion in the *Annals of Tropical Medicine*, published by the Liverpool School. Later on I shall be able to report on this when I have collected more data. A very luminous and suggestive paper by Sir Rubert Boyce, published in the *British Medical Journal* of September 14, gives details of experimental work on these lines. One drawback presents itself, to my mind, with regard to the Baganda. The whole race has an intense idiosyncrasy to mercury in any form. In treating syphilis, which is rampant in the country, we can rarely exceed the dose of only one-eighth of a grain of grey-powder, bis. or ter. die. A single grain of calomel may, and 5 grains almost certainly will, produce ptyalism, so that only small quantities of mercurial salts can be given.

Atoxyl is expensive. After the 10 per cent. duty has been levied, and the cost and carriage added, its price is about £14 sterling per pound. It has been suggested that the salt known as sodium amino-phenyl arsenate (prepared by Burroughes, Wellcome and Co.), at less than one-fifth the cost of atoxyl, viz., 3s. an ounce, and which has a similar chemical composition, may do equally well. I shall certainly give it a trial.

Though we cannot be too cautious in refusing to credit hasty assumptions of cure, I for one do not for an instant believe the problem is insoluble. Difficulties are made to be overcome, and are a powerful stimulus to research. Meanwhile, it is a source of legitimate pride that English physicians have taken such a big share in throwing clearer light on this mysterious disease. Forde and Dutton discovered the trypanosome, Colonel Bruce demonstrated the

important part played by the *Glossina palpalis*; Manson, Mott, and Thomas at home, and Greig and Minchin abroad, to say nothing of devoted Government medical officers, like Moffat and Hodges, have all helped, and are helping, on the cause. May speedy success attend their strenuous efforts.

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### Discussion on Sleeping Sickness.

Dr. G. C. Low said that with regard to segregation experiments were necessary to show how far the disease might be transmitted by infected animals. It was useless to remove infected natives if monkeys and other fauna were equally efficient hosts. Trypanosomiasis, like many other grave epidemics, seemed to appear at intervals; waves of sickness were followed by periods of comparative health; Nature, therefore, had a remedy, and much might be gained if our knowledge of this natural immunisation were more complete.

Professor Minchin spoke as to the anatomical characteristics of *Glossina*, and compared the fitness for the transmission of trypanosomes of various species of tsetse flies with that of stomoxys, and other suctorial insects.

Mr. Austen said that further experiments were required to settle the question whether other species than *Glossina palpalis* conveyed the disease, and a point of importance was the possibility of extension to South Africa. In Rhodesia, the only tsetse fly known at present was *Glossina morsitans*; but on the extreme north-western border of that territory there was a district where three species, *morsitans*, *palpalis*, and another, overlapped. A tsetse fly had been discovered in the Aden hinterland.

Dr. Sambon referred at some length to the biological affinities and distribution of various species of trypanosomes.

Sir Patrick Manson referred to the effects of mercury in the treatment of trypanosomiasis, and to its action in combination with organic compounds of arsenic. He said that on the whole the therapeutic position was encouraging, though some of the later remedies had been unsuccessful. A supply of parafuchsin, a drug which had been under observation and experiment at the Frankfurt Institute for Therapeutic Research had been sent him by Professor Ehrlich; but he was sorry to say his experience had not been favourable. He was satisfied that the human organism presented great natural resistance to the trypanosomes of sleeping sickness, which tended ultimately to die out if the patient could be kept going. Research was required to further elucidate the facts as to the life-history of trypanosomes and their hosts. Insufficient knowledge was at our disposal to enable us to supply Governments with responsible and authoritative advice as to preventive measures.

Dr. Cook replied to the arguments and criticisms of the various speakers, and said that the urgency of the matter was insufficiently appreciated. Research was no doubt imperative, but it would be useless unless it resulted in effective measures before there was no further need of them.

# DEMONSTRATION OF ORIENTAL SORE AND ITS PARASITE.

By Sir PATRICK MANSON, K.C.M.G., M.D., F.R.S.

(*Friday, November 15th*, 1907.)

Sir Patrick Manson said that, fortunately, he had an opportunity of demonstrating a typical example of the sore, which was variously known by several names, such as Delhi boil, Bouton d'Aleppo, etc. That this was a case of the disease had been proved by the microscope, which showed the characteristic parasite in enormous profusion. The Society was deeply indebted to the patient, himself a medical man, for his complacency and courtesy in having come there at much inconvenience to himself, and not without considerable suffering. He was a medical man, of about forty-one years of age, and had been working in Palestine for a considerable period. In June last he had to go to Baghdad, where he remained for nine months. On his return, and while at sea, some three weeks after he left that city, he saw what he thought was a mosquito bite on the back of his left hand: it was deep red, irritable, and itchy. Similar papules appeared at the same time on the dorsum of his left foot, and on the front of his right shin; they were dry and scaly, and gradually the scurf and scales accumulated, and, mixing with exuding lymph, hardened into crusts. After several weeks the crusts fell off, leaving a superficial ulcerated surface, which had an indurated base and elevated edges. This sore extended until it was 2 in. in diameter, when fungating granulations sprang up almost completely covering the eroded surfaces and preventing healing; but recently these granulations had be-

come dropsical and atrophied, and attempts at true cicatrisation had now begun to appear. That was to say, the disease had reached its final stage after lasting nine or ten months.

There were many interesting points in the history, the features, and the affinities of Oriental sore. First, it was protective against itself when it had run its course, the patient obtaining in most instances permanent immunity against the disease. Another characteristic was the uncertainty of the incubation period, which was sometimes short, so much so as to be almost an immediate sequence of infection and sore, in other instances running into many months. A lady missionary from Delhi whom he had recently seen developed Oriental sore in the South of France, five months after she had left the place of infection, and that circumstance, viewed in connection with immunity, was an interesting and important feature. Still another point of interest was that the disease occurred almost invariably on exposed parts of the body—on parts that were not generally covered by clothes—and that would lead to the inference that infection took place by some animal which could readily attack exposed parts. Insects, such as bugs and fleas, were thus ruled out, but mosquitoes and flies, for instance, might be effective carriers, as they were likely to convey it only to exposed places.

The disease, too, was inoculable. He himself had not been successful in inoculation experiments; but there was no doubt about inoculability. Apart from his own experience, that had been abundantly confirmed. Dr. Daniels, who was not likely to fail when care and dexterity in manipulation were required, had, however, made many inoculations with him without success, and he thought he had a good explanation for their failure. They had not succeeded in the experiments which they made upon dogs, rabbits, monkeys, and sheep, because the parasite of the

disease had passed the active stage, and was in an involuted or morbid condition. That, no doubt, too, was the explanation of the results which had recently been published by James. Deputed by the Indian Government to investigate the connection between the parasite discovered by Wright and the disease, Major James found, in Delhi, that only in a certain proportion of cases could he find in the scrapings of sores the characteristic organism. In many cases of undoubted Oriental sore, it was absent. As regarded the patient that evening, that too was the condition; whereas at first there were swarms of parasites crowding the cells and tissues, now they were extremely rare and difficult to find, and there could be little doubt but that these organisms were in the involution stage. But this mysterious malady had a still more interesting and noteworthy feature than all these. That was the relationship of Oriental sore to the deadly disease kala-azar. *Per se*, Oriental sore did not endanger life; though protracted, the tendency was to cure, and we could inoculate it without apprehension of serious evil. Kala-azar, on the contrary, never healed; it was most invariably fatal; and it would be, therefore, entirely unjustifiable to inoculate kala-azar into any human being. Yet the parasite of Oriental sore and the parasite of kala-azar were, so far as could be seen with the microscope, identical. He said morphologically, but perhaps not biologically, identical. And this fact gave rise to many questions and speculations. Were they of the same species? In their relations there was a unique opportunity of investigation, and perhaps of discovery. Of the connection between the Leishman-Donovan body and Oriental sore on the one hand, and kala-azar on the other, there could be little doubt; but it was only by successful inoculation experiments that the information about kala-azar, of which they stood so much in need, could be obtained. Assuming that the parasites were absolutely

identical, as Oriental sore immunised against itself it ought also to immunise against kala-azar. An opportunity to test that had recently presented itself at the School of Tropical Medicine. They had, in hospital, a case of Oriental sore, and at the same time a case of kala-azar, and he had been able to obtain the consent of the patient who was suffering from kala-azar to be inoculated with Oriental sore. Two students of the school volunteering to act as controls of the virulence of the inoculated matter were inoculated at the same time, but the result was failure; none of the inoculations took. Still, he would much like to get another opportunity to repeat that interesting experiment with an Oriental sore in an earlier stage. It was essential that one should have a thoroughly reliable virus, and at the same time a suitable case of kala-azar. The former he was endeavouring to start and keep going, by means of monkeys, etc.; the latter would, no doubt, in time present itself. If any who were present that evening could assist him with an opportunity, or avail themselves of an opportunity to test this hypothesis, the result, he believed, might encourage them to continue in a new line the attempts now being made to find a cure for kala-azar.

Again, was Oriental sore constitutional; was it a systemic disease; or was it one that, if the local lesion were cut out or destroyed, would thereby be cured? He considered that Oriental sore was a general infection, and that was probable, because, for one thing, a patient, on obtaining immunity, obtained general immunity. If the active sore was eradicated in one place, it would appear in another, and it could only be effectively cured when the parasite had run its course and reached the involution stage.

### Discussion.

Dr. G. C. Low asked if spirochætæ had been looked for in any cultures which had been made from Oriental sore. A pathological parallel was the parasite of ulcerating granuloma, for an organism which was either identical with that, or very like it, had been shown to occur in specific venereal sore, though it was not supposed to have a pathogenic function. The trend of opinion was indeed that all diseases of the granuloma type were due to spirochætæ, and he regarded it as at least probable that Oriental sore might safely be included in that category. It was not, he thought, proved that the parasite shown that evening was pathogenic of Oriental sore; it was perhaps only an extraneous parasite which had found a suitable medium. Sir Patrick Manson had not touched on the geographical distribution of kala-azar and Oriental sore, but an important point was that they did not correspond in that respect.

Lieutenant-Colonel Duncan said that he had been very much impressed by the success of a native remedy for Oriental sore on the northern frontier of India. He had seen more than one case, which had resisted hospital treatment for months, cured by the application, with pressure, of a flattened rifle-bullet cut to the shape of the sore.

Dr. Sambon said that formerly Oriental sore was considered merely a local disease, but the evidence was rapidly accumulating that it was constitutional. Immunity, he might say, did not preclude the possibility of relapse of a disease. Like malaria, Oriental sore might relapse even though immunity was being established by its own process. It should be borne in mind that exposure of the part was found to correspond with the lesion in other tropical diseases, such as pellagra, but a further and more important

peculiarity of Oriental sore was that it was frequently restricted to towns, country districts being free. He thought flies were scientifically, as well as popularly, incriminated; Oriental sore was locally called "fly disease," and it was known that parasites of the group to which the Leishman-Donovan belonged were often carried by flies, not merely suctorial flies, but also non-biting species. Leishman-Donovan, too, had been found in a sore on a dog, which to the eye, and to the microscope, was exactly similar to the Oriental sore of man, and it had been found many times in horses.

Dr. Hartigan thought Sir Patrick Manson had laid a little too much stress on exposure, because he had seen quite as many cases of Oriental sore on non-exposed parts as exposed parts.

Dr. Fremantle pointed out that the evidence for constitutional kala-azar was not strengthened by the theory of exposure; that indicated merely a local inoculation. The patient said that when sores appeared on his feet, he had been walking about in sandals, with bare legs, and it might be that this infection was, therefore, merely local, and akin to the granulomata met with in dissecting-room porters and other people who handled cadaveric material—an infection which, by the way, much resembled Oriental sore in other characteristics.

Dr. Newham asked whether the Finsen light or X-rays had been tried as a means of treatment?

Dr. MacDonald said he had seen a good deal of the disease in North Queensland, and he had believed it might be due to the bites of the small jungle leeches which, in such excessive numbers, infested most tropical forests. The Queensland sores were mostly on the face; oftenest on the nose and upper lip. He had seen them also on the arms.

The bites of the leeches, he thought, had a constitutional effect, and the disorder much resembled a disease met with in horses in Queensland, which was known as swamp cancer.

Dr. Frederick Johnson (the patient) said that at Baghdad, though most of the native children had the disease, the people were completely indifferent to it, and no steps were taken for its cure from beginning to end. They were equally indifferent as to the prevention of infection.

Sir Patrick Manson, in reply, said that Dr. Low's question as to whether the parasite was the actual cause of the disease was difficult of complete answer. The solution and proof was obviously to be obtained only by getting the parasite in pure culture, and then finding the insect medium by which it was conveyed. In the meantime, as parasitic growth was extraordinary, and as every endothelial cell was crowded with parasites seen in no other disease, it forced itself upon their belief, and they were justified in assuming, that they must be causative. As to spirochætæ, no doubt, after search, plenty of spirochætæ would be found in the tissue and discharges. They had been found in ulcerating granuloma by Wise in British Guiana, but investigators would get no nearer a solution in that way, on account of the difficulty of determining the relationship of any special spirochæta to the disease in which it was found. He had tried and failed to cultivate the parasite, because he could not keep his cultures sterile, and as they knew, bacterial growth was absolutely fatal to the kala-azar parasite. As to the distribution, kala-azar, it was well known, was a disease of low-lying riverine districts, and Oriental sore a disorder of dry, sandy countries; it was difficult to reconcile this with identity of the parasites, and in the meantime, he had no explanation to offer. Treatment by lead compress would be followed by

cure if the parasite had reached the involution stage; it would be useless, he believed, if the parasite was still active. As to Dr. Sambon's mention of recurrence, he did not recollect having seen reports of any such cases; but it was now believed that diagnosis of Oriental sore must be verified by the microscope. [Dr. Sambon here stated that the cases were reported by Dr. Vandyke Carter.] Perhaps the explanation of the fact that Oriental sore was often a town disease was that there is a large number of pariah dogs in many Eastern cities, and they are extremely liable to the disease. With regard to what Dr. Fremantle said, the lesion might be local, but the disease general; they had not to go far to seek parallels, for syphilis was one. As to the X-rays and Finsen-light treatment, he remembered one case of Oriental sore which had been diagnosed as lupus, and treated by X-rays with disastrous results. Severe inflammation had set in, and the patient's condition was much aggravated. Leeches, he thought, as suggested by Dr. MacDonald, could not transmit Oriental sore. There were no leeches in the sandy districts where Oriental sore was so prevalent; but it might be different with regard to kala-azar, which preferred damp, jungly, alluvial districts as areas of prevalence.

# A NOTE ON THE MORPHOLOGY OF *SPIROCHÆTA DUTTONI*.[1]

By the late J. EVERETT DUTTON, M.B. Vict.,

and

JOHN L. TODD, M.D. McGill,
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*From the Expedition of the Liverpool School of Tropical Medicine in the Congo*, 1903-04-05.

(*Read by Dr. SANDWITH, Friday, November 15th*, 1907.)

Breinl (1),[2] in the current number of the *Annals of Tropical Medicine and Parasitology*, describes the variations in the morphology of *Spirochæta duttoni* observed during the experimental study of that parasite made at the Runcorn laboratories of the Liverpool School of Tropical Medicine (2). His description is based upon the examination of dried and fixed preparations of blood and of organ juices of infected animals stained by Giemsa's modification of Romanowsky's method; films fixed while still wet were not found to show more detail. In his paper many new points are brought forward and antecedent observations made by others and by ourselves (3) are confirmed and completed. The first part of the present note is, with the exception of a few additions, an abstract of Breinl's paper.

*Spirochæta duttoni* is ribbon-shaped on transverse section.

[1] This note communicates the results of the last of the studies on which Dr. Dutton was engaged at the time of his death. In remembering his work all must admire the zeal which enabled him to accomplish so much that was good before he reached his thirtieth year; in remembering him we all regret his untimely death at an age when most men are first attaining their mature capacity for usefulness.—J. L. T.

[2] The parenthetical figures throughout the text refer to the bibliography at the end.

Though often wound in spirals it may be simply waved and hence may lie wholly in one plane; this can often be demonstrated in motionless parasites in fresh preparations. The spirochæta consists of a central core which stains a deep red and a surrounding periplastic sheath which stains a light pink (4). Both ends of the parasite are pointed, but one end (rarely both), through an extension of this sheath, is often prolonged into a flagellum-like process from which the central core is absent. No suggestion of peritrichal flagella (8) has ever been seen (4). The central core, or chromatic part, of the parasite frequently does not stain uniformly but shows more or less numerous irregularly-placed, unstained areas occupying the whole breadth of the parasite (5, 6, 7). Sometimes, especially in preparations taken just as the parasites are about to disappear from the blood, this fragmentation proceeds until the whole chromatic core is broken up into irregular granules; a clear area, of probably another nature, occurs quite constantly at the junction of the middle and a terminal third of the spirochæta.[1] The areas lying between the granules are usually unstained but may be coloured blue.

Spirochætæ frequently occur which possess either median or terminal knob-like swellings (7). Both median and terminal swellings stain red, though the latter are occasionally bluish, and both often contain granules of a dense red colour. A swelling in either situation is sometimes placed laterally and definitely outside the parasite, though still attached to it by a pink-staining band. Besides the usual slender parasites of uniform thickness others occur which gradually enlarge, either at the middle or towards one end, to twice the usual thickness (7); some of these forms perhaps represent a stage preceding longitudinal division. A small clear area containing a bright red granule is some-

[1] Breinl thinks that this may represent the site of fission in a future transverse division of the parasite.

times seen towards one end of such a parasite. The spirochætæ usually occur singly, but chains or groups of threes and fours are frequent. In the blood of heavily-infected animals, particularly if it has been shed for some little time, the parasites run together in enormous tresses or clumped agglomerations. (In such collections the parasites, with knob-like swellings, occur most frequently; they are also common in blood ingested by ticks.) Occasionally a parasite is either wholly or partially rolled up into a tight coil. The spirochætæ are usually extracellular; rarely in blood taken just before a crisis they are found within both red and white (9) cells.

Perhaps the most usual method of multiplication of *Spirochæta duttoni* is by transverse division. The parasites increase in length, and become thinner at about the middle. This thinner area is gradually more and more drawn out until the parasites separate. Longitudinal division also occurs and seems to be most frequent at the end of an attack when the parasites are disappearing from the blood. Before dividing, the parasite increases considerably in width. It then splits, commencing at one end, so that definitely Y-shaped forms are produced; these forms are not seen very frequently, however.[2] It therefore seems possible that longitudinal division may not be a common method of multiplication. An appearance, which Breinl suggests may represent a conjugation, occurs very rarely in the blood. Two parasites lie side by side and touch at one or more points. One stains a uniform red, the other is a light blue and apparently has no chromatic core, but possesses a varying number of red granules placed at the spots at which the two parasites are in apposition.

Striking changes occur in parasites contained in the

[2] Y-shaped forms were particularly numerous in the juice obtained by splenic puncture just at the end of an attack in a patient who died from the disease.

organs, particularly the spleen, bone marrow, and liver. In preparations taken just before a crisis, but while the blood is still swarming with parasites, many of the spirochætæ in spleen and bone marrow, rarely in the liver, are tightly coiled up. Some of them are swollen, the majority are more slender than usual and form skein-like coils which seem to become more irregular as the crisis approaches. Many of such parasites are engulfed by the phagocytes of the spleen, and at the crisis the spleen cells are gorged with degenerated spirochætæ of this type. (In animals from which the spleen had been removed, an analogous process took place in the liver.) A very few similarly coiled parasites undergo a remarkable change. They may be much fragmented or may retain their original outline. They lie placed in a bluish-staining ground substance, within a definite cyst wall, and so form a sporocyst-like body of about the same size as a red blood-cell. These forms may be seen in the blood after all other forms have disappeared. They may undergo further changes: the outline of the coiled parasites becomes more indistinct and the chromatic core fragments until only the cyst, filled with red granules, remains. It is impossible to trace the further development of these forms in blood and organ juices stained by Romanowsky's method because of their great likeness to blood platelets. In the blood and organs of infected animals, and also in the blood contained in the alimentary canal of ticks, blue bodies, about 3 $\mu$ in diameter (in the tick as small as 1 $\mu$) with red central granules constantly occur. Of their nature we can say nothing save that we could identify them with no normal structure.

In only one particular have our observations led us to conclusions in disaccord with those reached by Breinl. We believe that a certain appearance, also noted by Leishman (10), definitely shows the presence of an undu-

lating membrane in some, not all, of the spirochætæ. Breinl maintains that the appearance is merely an illusion produced by the flattening of the spiral organism on the slide. The question can only be definitely settled by the indisputably obvious demonstration of the membrane by some special technique superior to that employed by us. We have seen what we believe to be an unmistakable undulating membrane in spirochætæ in blood taken directly from the circulation of men and animals, and in blood from the alimentary tract of ticks (*Ornithodorus moubata*) previously fed upon infected mammals. The blood was examined either dried, fixed and stained by Romanowsky's method, or fresh in moist preparations. Some of the moist preparations were examined at once ; to others varying strengths of saline solutions, tinted with neutral red, were first added ; with others a weak solution of gelatine was mixed in order to retard the movements of the parasites. By each of these methods the undulating membrane has been demonstrated in either dead or living parasites. It is not seen in every spirochæta, although it can usually be found in some parasites in every slide examined. It is perhaps most easily seen in dying parasites which have been injured by the addition of a salt solution of unaccustomed density to the blood containing them ; in some such injured parasites the membrane can be seen hanging, partially free. Perhaps for a similar reason it was also very well seen in blood from the gut of ticks. In any case it is only by the most careful examination of specimens with the highest powers[1] that the membrane can be perceived. When it occurs it runs the whole length of the body, and it is occasionally manifest that some of the unstained areas in the spirochæta are due to the fact that it remains unstained at the points where its body is crossed by the membrane.

[1] Zeiss, apochromatic objective, 2 mm. aperture 1.4 ; Ocular No. 18 or 27.

Various circumstances have prevented us from making an exhaustive study of the changes undergone by *Spirochæta duttoni* in *Ornithodorus moubata*. Many preparations, made at various periods after the infecting feeds, of material from different parts of the bodies of infected ticks have, nevertheless, been examined. The appearances we mention were noted in preparations of either alimentary canal (usually receptaculum) or Malpighian tubules. We merely record our observations, and do not attempt to give a complete account of the development of the spirochæta.

Besides obviously degenerating parasites all the variations in form seen in infected animals also occur in the tick. The parasites in blood from the receptaculum, which was ingested twenty-four hours previously, are differentiated into two more or less distinct types—one slender, the other stout. Besides these two types a third form, a huge parasite of twice the usual length and thrice the usual breadth, is seen occasionally. Parasites, usually coiled within white and red cells of the ingested blood or within the lining cells of receptaculum or tubule, are not infrequently seen. The coiled skeins and encysted forms are frèquent. The most marked feature of the spirochætæ in the tick is, however, the extraordinary number of ways in which their chromatin may be fragmented. Parasites of all types, without exception, are affected. The granulations may be either coarse or fine. In one or two instances the granules of a finely-fragmented, extracellular parasite are divided transversely, so that its chromatin is represented by a row of paired granules. The granules of the encysted parasites are usually single. In others, perhaps at a more advanced stage, they are double, and the parasite then consists of a faintly blue or reddish spherical matrix about 5 to 7 $\mu$ in diameter, in which are embedded perhaps a score of pairs of deep red

granules. Many such bodies have been seen in preparations from the Malpighian tubules of an infected tick. The further development of these bodies is problematical; it is certain that some of them burst, and that their granules are thus freed. In stained specimens it is apparently possible to trace the development of each single granule to a comma-shaped mass of chromatin and of these comma forms to small spirochætæ. This possible development must, however, be confirmed by the continuous observation of fresh specimens. Breinl recalls the fact that blood which contained spirochætæ is still infective after it has been passed through a Berkefeld filter. He surmises that this may be explained by the presence of the granules described above, and he suggests that the above cycle of development, which we think occurs in the tick, may also take place in the mammalian host.

While many of the appearances described remain unexplained, it seems possible from our observations to draw the following tentative conclusions concerning the development of this parasite:—1. *Spirochæta duttoni* may multiply by direct division, longitudinally or transversely; 2. It may possibly multiply also by a process in which the fragmented chromatin of an encysted parasite is extruded in granules, each of which may subsequently develop into a new spirochæta.

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# A VISIT TO THE TROPICAL SCHOOL AT HAMBURG.

By F. M. SANDWITH, M.D., F.R.C.P.,
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(*Friday, November 15th*, 1907.)

At the end of the Fourteenth International Congress for Hygiene and Demography, held in Berlin last September, some five hundred of us were invited to visit Hamburg, where all the various institutions were thrown open for our inspection. We visited them in five groups, and I took the long-desired opportunity of spending two days at the Hamburg Tropical School. This school, which is officially called the Institute for Diseases peculiar to Ships and to the Tropics, was created in consequence of the need which the State of Hamburg felt for practical and scientific research into marine and tropical diseases. The site was well chosen, because the sanitary service of the port furnishes excellent material for the work. The Institute was also founded to give special instruction to medical officers of the mercantile marine service.

In the year 1900, following close upon the creation of the London and Liverpool Schools of Tropical Medicine, the old Seamen's Hospital at Hamburg was converted into the Institute, and a new two-storied hospital of fifty-six beds was built in the same compound for the reception of tropical patients, especially seamen.

The Imperial Government of Germany determined about the same time to establish a training school for officers of

the Colonial Medical Service, and instead of opening this in Berlin they formed a coalition with the State of Hamburg, by which they, while retaining certain rights and granting money in aid, ensured for their naval officers tuition in the Institute and adjoining hospital. The Institute at Hamburg, therefore, has the advantage of officially belonging both to the Imperial Government and to the State of Hamburg, and when money cannot be obtained from one of these sources it is often easy to procure it from the other. This doubtless accounts for the fact that the financial position is never strained, and though I have made many inquiries I have not yet succeeded in finding anyone who could tell me what the annual budget amounts to. The whole establishment is under Government and Municipal control, and is not dependent, as our English Tropical Schools are, on the benefactions of the public. I should also mention that grants in aid seem to be obtained from the War and Marine Ministries.

Another advantage which the Hamburg Institute has is that it was considered important from the very beginning that it should be in close touch with the sanitary service of the port, and therefore Dr. Nocht, the port medical officer of the city, was appointed Director of the Institute, and has his chief office in that building.

In 1906 the Institute was found to be too small, and was therefore enlarged ; zoological laboratories were added, under the care of the late Professor Schaudinn, who has now been succeeded by Dr. von Prowazek.

## Description.

The Institute stands on a hill in the centre of the docks, about half an hour by tramway from the heart of the town. On the ground floor are the Director's room, other offices, and a research laboratory for four workers. On the first floor is the library and reading-room. Two lady librarians

are in charge, the books are catalogued by the card system, and recent additions have been made to it by buying Professor Schaudinn's books. The library subscribes to seventy-seven periodicals, and exchanges Transactions with many institutions; the library expenses amount to about £250 a year. The laboratory, built in 1906, is 58 ft. long by 19 ft. wide, with six large windows, and is fitted with gas, water, and electric light for twenty-four students.

The museum is arranged round the sides of the laboratory, and a good epidiascope exists. On the same floor there is a chemical laboratory of two rooms, another room for experimental operations on animals, without legal restrictions, and some smaller laboratories. On the second floor is the protozoology laboratory consisting of three rooms for five workers, a tropical room kept at a temperature varying between 77 deg. and 86 deg. Fahr., and a relative humidity from 60 to 70 per cent. This serves for the breeding and development of mosquitoes, flies, ticks, snakes, etc. The ticks I saw were kept between two layers of tripod tables, enclosed in glass jars, with lids of wire gauze weighted with lead, and enclosed in tins of double saucers, containing petroleum or vaseline.

A museum of tropical hygiene is in course of arrangement, but all that was shown to us was a model of a mosquito-protected bed. There is a club-room for students, but there are no residential chambers for them in the building.

In the basement there is a room for micro-photography and a dark room with red walls, an automatic washing-tray, and an electric ventilator for drying plates.

On the ground floor of the hospital there are two wards, each of fourteen beds, one of which is supposed to be reserved for coloured patients, but at the time of my visit there were several Europeans suffering from typhoid fever in the native ward. Each typhoid patient was surrounded

by a large mosquito net to minimise infection. A small operating-room was on this floor also.

On the first floor there was another ward of fourteen beds, a smaller ward and several rooms for one or two patients, with large terraces overlooking the harbour and garden. The second floor is devoted to the sisters' quarters.

The kitchen of the hospital is in the large seamen's house in the same compound, which is a sort of club for sailors. The patients' fees are as follows: those in the first class pay 7*s.* a day if they come from Hamburg, or are employés of the Colonial Office, while ordinary patients pay 10*s.* In the second class they pay respectively 4*s.* or 6*s.*, and in the third class 2*s.* 6*d.* or 3*s.* 6*d.* per day.

The total number of patients admitted to this hospital from 1901 to July, 1907, is 3820, which gives an average of about 588 per annum. The stables consist of two wings, somewhat small and ill-ventilated. The dogs had had their inferior laryngeal nerves cut to prevent their barking. Near the stables is a mosquito house, with a cemented tank, for breeding different kinds of mosquitoes.

### Course for Students.

This is both practical and theoretical; there is a short course held twice a year, lasting only three weeks, for the medical officers of ships. During this time they are taught the hygiene of vessels, including food and water examination, care of the sick on board, the duties of a ship doctor, such as the prevention and stamping out of infectious diseases, quarantine and disinfection, and practical information about malaria, sleeping sickness, kala-azar, yellow fever, scurvy, beri-beri, Malta fever, plague, cholera, typhoid, dysentery, and helminthology.

The tropical course lasts ten weeks, and is also held

twice in the year, special attention being paid to protozoology. The syllabus comprises the histology and pathology of blood, mosquitoes, malaria, blackwater fever, trypanosomes of men and animals and their carriers, piroplasmosis, kala-azar and Delhi boils, tick fever and relapsing fever, syphilis, yaws, verruga Peruana, spirillar diseases of domestic animals, yellow fever, dengue, filariasis, ankylostomiasis, bilharziasis, paragonimiasis, parasitic flies, poisonous animals, dysentery, liver abscess, sprue, Madura foot, beri-beri, scurvy, leprosy, plague, Malta fever, cholera, typhoid fever, hygiene of ships, tropical hygiene, care of vaccine lymph in the tropics, cattle plague, bovine tuberculosis and horse sickness. Veterinary surgeons are sometimes sent to attend part of the course by the Colonial Office, and others join privately.

Since the beginning, 349 graduates have passed through the Institute, 115 of these being colonial officers, including seventeen veterinary surgeons: thirty-one have been naval doctors, seventy-nine doctors of the merchant service, eighty-three private practitioners, and the remaining forty-one were foreign students. Among the latter class many nationalities have been represented: Hungarians nine, Dutch nine, Swedes, Swiss, Russians, Japanese, Brazilians, Bulgarians, and one each from Belgium, England, and Australia.

## Fees.

Three guineas are paid for the short course, and double that sum for the tropical course, while the Hamburg doctors pay no fees.

The clinical material in the hospital during the last six years has comprised 1578 cases of malaria, eighty-three cases of dysentery, mostly amœbic, seventy-one of blackwater fever, 145 of beri-beri, two cases of trypanosomiasis, besides patients suffering from various worm infections, yaws, liver abscess, sprue, and leprosy.

The students in the laboratory are given opportunities of making for themselves a collection of microscopical specimens of most of the common tropical diseases. They are instructed in the examination of meat at the Hamburg slaughter-house. Advanced students are welcomed for research work in the Institute. Special lectures are given to nurses and missionaries who are proceeding to the tropics, and some lectures have been given to lay audiences. Illustrated postcards are published to instruct lay residents in the tropics.

The Institute has so far not been able to send any scientific expeditions to the tropics, but hopes are entertained that this will be possible in the near future. I may, however, mention that in 1904 Drs. Otto and Neumann proceeded to Brazil in order to study yellow fever, and in the following year Dr. Otto went to Togo, West Africa, for the same purpose. In 1906 Drs. Fülleborn and Mayer travelled in Egypt, Ceylon, India, and East Africa, to study tropical diseases, and all these journeys have been amply utilised to enrich the pathological museum.

For teaching purposes lantern slides are largely employed, and there are some excellent coloured diagrams, which are shortly to be reprinted for sale.

In order to give every facility to students to study helminthology, sections of worms are placed vertically in stands with electric light behind them ; the student examines these at leisure with a magnifying glass. There is a splendid collection of microscope slides for demonstration purposes, and a good supply of photographs. Tsetse flies are sent to Hamburg from the tropics in the pupa stage, so that they develop into mature insects either on board the ship or after arrival at the Institute.

I had the advantage of attending an epidiascope demonstration given by Professor Fülleborn, when due honour was paid to the various workers of this country who have

made discoveries in tropical medicine. Dr. von Prowazek showed us newly-discovered protozoa in trachoma from Sumatra, and Herr Giemsa, the chemist, explained to us by diagrams, the Nocht-Giemsa apparatus for killing rats on ships. The gas derived from combustion of coke requires twelve hours to disinfect a large passenger steamer. We saw some rats killed with this gas in half a minute in a bell glass, and it is believed that all rats are destroyed on a steamer in about ten minutes after exposure to the gas. The gas consists of about 8 per cent. carbon monoxide, with a little carbonic acid, and some 70 per cent. of nitrogen.

The cost of disinfecting a moderate-sized steamer is £7 10*s*. In addition to the compulsory destruction of rats on vessels arriving from a plague-infected port, owners often request that rats, when a nuisance on a ship, should be killed by this method. The Giemsa apparatus does no harm to any cargo, and has this great advantage over Clayton's sulphurous-acid process.

When a ship arrives from a plague-infected or suspected port the rats on board are examined, especially when it is reported or discovered that any have been found dead or dying. For this purpose sample rats are sent to the Hygiene Institute, of which Professor Dunbar is the Director. If the *post-mortem* examination of the rat shows plague or anything suspiciously like it, all the rats on board that steamer are destroyed before the cargo is landed. The crew are disembarked in a special place before fumigation begins, and are not allowed to return to the ship until the air has been tested by animals in cages to make certain that no dangerous gas-poisoning remains. The Nocht-Giemsa apparatus introduces gas into the hold and on completion of the process exhausts it again. Though the gas kills rats successfully, it does not disinfect the cargo, which must therefore be carefully removed by

specially-protected workers into rat-protected stands. When a case of human plague has occurred on board, the clothes and bedding are sterilised and the water used during this process is boiled before it is allowed to flow into the Elbe.

On the fifteen ships first submitted to this process, 2845 rats were killed, of which 150, or 5.2 per cent., were plague infected; in the same ships 184 mice were killed, of which only one was proved to have plague.

It would have been impossible for me to have profited so much by my two days' stay at Hamburg if it had not been for the great courtesy of Dr. Fülleborn, who most kindly devoted some hours to explaining the methods of work to two or three of us.

I have, I hope, shown that the Hamburg Tropical School is full of vitality, and doing excellent work. Enjoying, as it does, liberal financial support from the Government, it is already in a more favourable position than the Tropical Schools of London and Liverpool, and England must look to it that she be not left behind in the friendly competition for increased knowledge in the vital subjects of tropical medicine and hygiene.

# EXPERIENCES OF ANKYLOSTOMIASIS IN AUSTRALIA.

By T. F. MACDONALD, M.D., Geraldtown, North Queensland.

(*Friday, December 20th*, 1907.)

When Professor Sandwith honoured me by suggesting that I should contribute a short paper to-night on the subject of Ankylostomiasis, I very readily embraced the opportunity of securing your expert criticism upon some of the Australian manifestations of that disease.

By way of introduction I may mention that tropical medicine, as far as Australia is concerned, has recently assumed an entirely new importance, owing to the advent of workers of the white race in tropicultural industries of North Queensland. There, within the last few years, South Sea Island, and other coloured labour generally, has been replaced by workers of our own people in fulfilment of the national ideals of "White Australia"; and, for the first time in history, the Caucasian skin has been presented with an opportunity of proving its power to survive under democratic conditions in tropical lands. Tilling and milling of sugar cane, previous to our Queensland experiments, has never been done by other than coloured races. The bearing of this departure from ancient custom is obvious. With millions, instead of hundreds, of white people in the tropics, we who subserve the best interests of the people, by striving to keep them in good health, may count upon a much to be desired increase of public support. Institutions for the study of tropical science, such as we only dream of now, will, I sincerely hope, multiply with truly tropical luxuriance. To some extent I am responsible for

the later positive issue of the great controversy in Australia, as to whether white people could or could not do all necessary work in tropiculture; my reports and representations to the Federal Government being quite the first to contain a shade, at least, of scientific evidence in support of the affirmative propositions. The moral obligation and responsibility thus incurred lent particular interest to my efforts to cope with Australian tropical diseases, especially with that of ankylostomiasis. That insidious enemy of health I found deeply rooted and flourishing among the inhabitants of the Johnstone River district (which is situated between Townsville and Cairns somewhere about the 14th degree of S. latitude), when in 1895 I commenced practice there, after having visited Egypt, China, and Japan, and learned some of the main features of tropical work.

Planted in the heart of a jungle of scrub sixty miles square, with a rainfall of 200 ins., frost unknown, here were ideal conditions for the incubation of parasites, and here *Ankylostomum duodenale* spread from family to family with incredible swiftness.

Having determined the parasite, secured from a patient who dwelt in a house wherein three deaths had taken place prior to my arrival, and which was reported as Bright's disease, with dropsy, I took steps to have my diagnosis confirmed, sending specimens of the worms to Brisbane, Sydney, and Melbourne, with the result of confirmation of my view as to their identity.

Link by link I picked up the chain of evidence of the serious nature of Australian infection by this disease. Reported from Cairns, Townsville, and Brisbane, the worm had been looked upon more or less as curiously rare.

After my first reports as to the magnitude of the infection in my own district, Dr. Tom Bancroft, of Brisbane, was good enough to keep me posted with all the latest papers

on the subject, among others those of Dr. Looss, of Egypt, after which the advice to "use earth closets and wear boots" became an aphorism.

More and more I became animated with the desire to stamp out the scourge, which I could see plainly was sucking the heart's blood of the whole community. In vain, however, I appealed to municipal authorities and to the Queensland and Federal Governments for assistance.

The Chief Medical Officer of Health in Queensland received contradictory reports from various local Health Officers, and unfortunately did not himself visit the affected areas, so put my public statements on the matter down to gross exaggerations—as if it were possible to exaggerate the seriousness of conditions where a community of some 5000 people in tropical environment is infected with ankylostomiasis to the extent that cases were found in almost every square mile of the district, where thymol had to become almost routine treatment among plantation labour, and where every farm provided victims. Ninety per cent. of the scholars in one school alone were found to be infected with ankylostomes when in one day I subjected the whole school to treatment, and these were white children.

The happy thought struck me to enlist the sympathetic assistance of the Bishop of North Queensland, who, much to his credit, entered heart and soul into my proposition that a School of Tropical Medicine or Research Institute should be established in Townsville, the capital of our tropics.

Quite recently the Federal Government has voted the necessary funds for that purpose, and I shall await with much interest the contradiction or confirmation of my statements concerning ankylostomiasis in Australia.

As to the disease itself, among children the most pronounced symptom was the extraordinary appetite displayed

for eating earth, not in casual manner, but under the impulse of an irresistible craving. Babies would pick dirt from the seams on the floor or from boots carelessly left uncleaned; older children frequently expressed a desire to suck stones, preferring such luxuries to sweets. Adults did not eat earth. but developed abnormal delights in pickles, curries, and alcohol.

Quite the most interesting feature of the clinical aspect of ankylostomiasis was to me the recognition of a series of, as far as I know, unrecorded psycho-pathological symptoms characterised by, so to speak, a "spectrum" of deeper and deeper immoral qualities, growing and strengthening with the progress of the disease.

It is difficult to determine whether there is *per se* vicarious appetites of the moral and mental systems parallel to those of the physical order; or if the moral and mental aberrations to be observed in an outbreak of this disease are merely accidental. My own belief is that they are as definite symptoms of positive nerve poisoning as anæmia is of blood destruction. But, just as good food and general hygienic environment retards the progress of physical symptoms, so, too, moral surroundings will retard the growth of, and probably counteract tendencies to degenerate habits. Children love to eat earth so much that they disobey their parents' commands to refrain from indulgences of that sort. *Disobedience* thus forms the first colour in the moral spectrum. *Cunning* soon follows; and, accordingly as punishment is administered or not, *lying* becomes a distinct symptom. The next stage of advancing immorality is a pronounced love of *stealing*. Children have told me that they enjoyed stolen food much more than that obtained in the usual manner.

Later stages of degeneration are of the sexual order. Schoolmasters have consulted me as to the cause of general demoralisation among school children. Thymol provided

a key to the difficulty, with, I am glad to say, happy results. One boy who stole money from my house on two occasions, and confessed to further depredations, regained normal feelings of honesty and uprightness by treatment also with thymol.

A young girl of highly imaginative character, under the influence of the disease, forged letters to her mother, a widow, setting forth proposals of marriage from supposed admirers, coupled with the information that she must be kind and lenient, even indulgent, to her charming little daughter! This charming little daughter went so far with her forging propensities that it was necessary to take steps to stop her. I recommended thymol as a first "punishment," which resulted in the expulsion of quantities of ankylostomes; and, later on, regeneration of character.

Another little maid of nine summers developed a faculty for finding half-sovereigns. One of those coins happened to be one which I had marked and missed. Nothing was said to the child, but thymol was exhibited, with the usual result, and careful watch kept for developments. In order to study her case, it was arranged that the girl should stay with my servants for some months: and from being quite the "naughtiest" girl at school, she grew into a really bright and loveable child.

Two factors are necessary, in my opinion, to produce effects of this nature: (1) a weakened physiology of the victim, and (2) an exciting nerve toxin. This part of the work I intend to follow up in one of the laboratories of Europe, and possibly may have more to say on the subject at some future date. At present the matter is too much in theory; but still, clinical indications are very strong in showing that there may be a rational explanation of child immorality.

An editor of a local paper, who was at the same time a

member of my hospital committee, "blabbed" to the press some of the information tabled from month to month on the subject. The result brought a flood of letters to me from all parts of Australia—letters which proved clearly that ankylostomiasis was fairly distributed over the continent, although not with such virulent outbreaks as those I was personally in touch with in Queensland. To test several of those distant cases I sent thymol, and received back in due course specimens of the required nematode worms.

Australia has been infected by ankylostomiasis through three distinct channels: by South Sea Islanders, Arabians, and Italians. My brother, Dr. W. C. C. Macdonald, of Ingham, Queensland, relates how an Italian went to him from Pisa Hospital, where, rather strange to say, his case had not been diagnosed. The man had relations at Ingham, who had been victims of the disease; and hearing of their experience, he diagnosed himself and set out for Queensland, and in due course returned with a bottle of ankylostomes. In all probability the natives of Australia were affected by this parasite before the advent of the white race; for it may be found among them now, and several outbreaks have been recorded.

It is comparatively an easy matter to treat individuals, but how to stamp out the disease in communities is the question we still have to face. In my district, being medical officer to the General Hospital as well as to surrounding plantations, with special local hospitals, I had a good opportunity of testing the strength of this disease when thoroughly engrafted into a community. During this time the farming class passed through a transitional period from almost poverty to great prosperity, and advantage was taken when new farm homes were being built to treat all members of the home by thymol, within a week; to disinfect as thoroughly as possible; and, finally,

to burn old houses. In this way many centres of infection were for the time being obliterated; but, alas! how quickly new centres arise! Still with reasonable assistance from society, whether by way of Government grants or voluntary contributions, the outlook is far from hopeless.

The treatment of *definite contacts*, whom I should classify as those staying under the same roof, is an important matter in checking the development of community infection.

A splendid factor in my own efforts was the assistance rendered by the children, who realiy knew more of the disease than wiser people would imagine. They gradually became quite learned in symptomatology and entered enthusiastically into detection work. In this manner earliest symptoms were often noted in very young children, and information spread from one child to another. A child who has once passed through the pangs, sorrows, and miseries of an attack of ankylostomiasis develops keen sympathy with others so affected. There is an unwritten tragedy in their young lives never to be forgotten.

It is much to a child to lose all desire for food and play, to grow weak and breathless, and to swell with ascitic fluid—to see other children pine and die; to sit in listless groups; and, in addition, often to suffer punishment for weakened memory; the only excitement in their lives to be of a morbid nature.

The faces of children victims of ankylostomiasis can never be forgotten: a world of woe is engraved thereon in saddest lines; and those unhappy pictures nerve one on to unceasing efforts to do one's own part, and to arouse society to a sense of its responsibility in the great fight against disease, more especially where duties are apparent and the issue plain, as it is in the battle against ankylosto-

miasis, which is not a local matter, not even national; it is most emphatically an international question.

In conclusion, Who knows what nations may not have been swept away in the slow waves of time, done to death by the insidious poison of this subtle disease? Who knows but that the original serpent of evil was none other than *Ankylostomum duodenale?*

# PHYSALOPTERA MORDENS: A NEW INTESTINAL PARASITE OF MAN.

By ROBERT T. LEIPER, M.B., F.Z.S.

Helminthologist to the London School of Tropical Medicine.

(*Friday, December 20th*, 1907.)

DURING a recent visit to Uganda, for the purpose of making a reference collection of the helminthic parasites of that country for the Museum of the London School of Tropical Medicine, I had the opportunity of investigating in the Research Laboratory at Entebbe a curious "ascarid"-like nematode that had been obtained at a post-mortem of a native who had died, in the hospital, of sleeping sickness. From my examination of the material, which had been very kindly placed at my disposal by Dr. Gray of the Royal Society Sleeping Sickness Commission, I was able to determine that the specimens belonged to the genus Physaloptera, a genus that had hitherto been found but once in man. It was not possible at the time to say, however, whether the specimens represented a new species or belonged to that founded by v. Linstow in 1902 for those discovered in Southern Russia, and named by him *Physaloptera caucasica*. Since my return to London I have been able to compare the details of organisation of the African form with those given by v. Linstow for *P. caucasica*, and with the other forms that appear in literature, and to satisfy myself that the form met with in Central Africa presents several notable distinguishing characteristics.

The genus *Physaloptera* has representatives in the four

higher groups of the Vertebrata, but has not yet been found to occur in fishes. There are in all fifty-five known species, of which twenty-two occur in the Mammalia.

The genus is, however, a very old one, and contains a number of species that depart very widely from the type as defined by the type-species *P. clausa*. Further, the characters by which many of the species in the genus have been defined are really of generic value, so that, in the absence of the original material, there is little beyond a measurement or two upon which the specificity of several species remains. This is unfortunately true of the description of *Physaloptera caucasica* with which the present species required to be most carefully compared.

The generic characters that appear to be constant for the species of the genus *Physaloptera s. str.* may be summarised as follows:—

The general form resembles markedly that of the genus *Ascaris*, but in the male the cuticle at the posterior end of the body is flattened out to form a somewhat heart-shaped "bursa." In both sexes the cuticle overreaches the mouth structures, forming a sort of cuticular sleeve or collar; it is, moreover, unmarked by the regular transverse striations seen in other forms, although, of course, in contracted specimens, certain transverse groovings appear adventitiously.

The mouth is surrounded by two large fleshy lips, situated laterally. Each lip bears two large submedian flattened papillæ, and is surmounted by a cuticular prong called the external tooth. Beneath this, in the surface in contact with the opposing lip, is a cuticular fold that shows varying form with the different species, and is known as the inner tooth.

The bursa in the male presents a series of papillæ which fall into two classes, pedunculated and sessile. Typically,

there are in each species ten pairs, four pedunculated and six sessile. The pedunculated series lies in a longitudinal row, outside the sessile papillæ. Though the number appears to be constant for most, at any rate, of the species, the relative position of the papillæ to one another seems to be of specific value.

The vulva is situated in the anterior half of the body, and the ova show an exceedingly thick smooth shell, that in itself is highly characteristic.

The species obtained in Uganda and which we shall call *P. mordens*, like *P. caucasica* presents all the characters above defined, as those of the genus in a restricted sense. Its size is very considerable, the male measuring 30 to 50 mm., the female 40 to 55 mm. In shape and general appearance it resembles an immature *Ascaris lumbricoides* —so much so as to lead me to conclude that it may have been frequently overlooked as such in the past, just as *Ascaris mystax* and *Ascaris marginata* have been. To the latter, also, it bears a striking resemblance, the bursal expansion at the tail of the male resembling the cuticular alæ that are present at the head of these two species.

The breadth of the worm is considerable, and varies from two to three millimetres. The body tapers very gradually anteriorly, and ends truncately; posteriorly it tapers rapidly in the female to a sharp tip, lying more in line with the ventral than the dorsal surface, and giving therefore the posterior portion of the worm a somewhat rounded appearance on the whole. In neither sex does the cuticle form posteriorly a præputial fold around the posterior portion of the body, as it does in some species in addition to the characteristic anterior bulging seen in all. The cuticle shows adventitious groovings that have no constant relation to one another. In this species, however, it is not absolutely without structure, exceedingly fine radiate striæ being noticeable.

The mouth parts exhibit several interesting details of structure that are unfortunately unnoted either in the description or in the figure of the corresponding species *P. caucasica*. The two large fleshy lips of *P. mordens* bear in addition to the pair of touch papillæ and the horny "external tooth" common to these forms, a pair of cuticular knobs that jut out and lie in apposition to one another at the outer and posterior limits of the transverse fissure of the mouth, on either side (Fig. 2).

Further, the cuticular fold known as the "inner tooth," lying in the median line on the inner aspect of the lip and overhung by the horny external tooth, presents a sharp-cutting edge towards the lumen of the mouth, and projects forwards, tapering to end anteriorly in a single lancet-like tip.

Both lips are bound together posteriorly by a thick encircling cuticular band, where the cuticle leaves the surface of the anterior part of the body to form the præputial-like fold characteristic of the genus. At a distance equal to three times the transverse diameter of this band, measuring backwards therefrom, are to be found the cervical papillæ.

The alimentary canal shows the usual divisions into œsophagus, intestine, and rectum. The œsophagus, in the specimen figured, measured exactly one-sixth of the body length. It shows transverse muscular striations, and becomes only very gradually more broadened towards its intestinal opening. The chyle intestine commences with a lumen only slightly greater than the external circumference of the terminal part of the œsophagus and gradually becomes narrowed during its whole course to the short rectum, at its junction with which one finds the large unicellular masses that have been noted at this situation in many other groups of nematodes (Fig. 1).

The anus lies on the ventral surface at a distance of

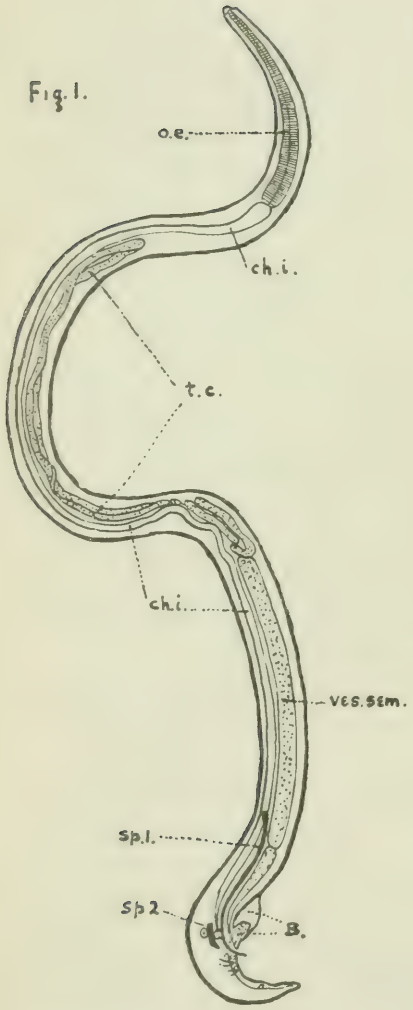
one-eleventh of the body length, in the male, from the tip of the tail.

The bursa in the male is oval in outline, tapering posteriorly. The papillæ are in number and in general disposition according to the type of the genus, but in minuter detail of the relation of the individual papillæ to one another the following points are to be noted. The external series of four pairs of pedunculated papillæ do not lie in one straight longitudinal series, but are disposed so that the first and fourth pairs lie considerably within the middle, *i.e.* second and third, pairs (Fig. 4).

Of the sessile series of papillæ the only point to note is that, in contrast with the arrangement described and figured for *P. caucasica*, the most anterior of the three hindermost pairs of sessile papillæ on the tail are much more widely separate from one another than are the members of the two succeeding pairs.

As in *P. caucasica* and many other species, the ventral surface of the bursa has a peculiar "rough-shod" marking of the cuticle that lies between the most anterior pair of papillæ and the second pair from the posterior end, the cuticular markings extending between this second last pair and gradually disappearing, doing so more quickly from the sides than in the centre line.

The male genital system consists of three distinctly-defined portions of the long single genital tube and a pair of very unequal spicules. The genital tube lies almost entirely on the ventral aspect of the body cavity, keeping the alimentary canal, in its whole length, on its dorsal aspect. The ejaculatory duct, the most terminal part, measures about one-fourteenth of the body length and shows well-developed musculature in its walls. The portion immediately succeeding, viz., the vesicula seminalis, is a wide and lengthened tube having a peculiarly reticulated wall, and extending forwards without kink or coil for a distance



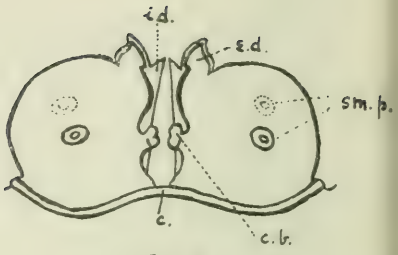




Fig. II.

Fig. III.

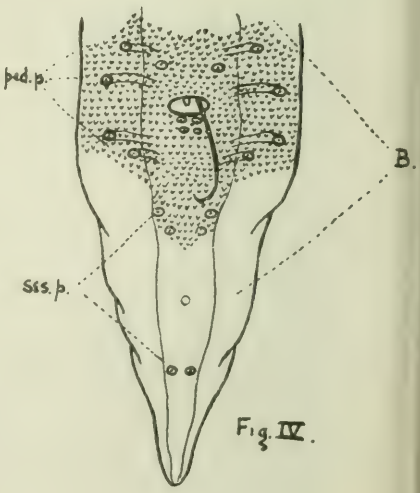



Fig. IV.

PHYSALOPTERA MORDENS, N.SP.

## EXPLANATION OF PLATE.

Fig. 1.—Camera-lucida Drawing of an adult male *Physaloptera mordens*.

Fig. 2.—Mouth parts of *P. mordens*.

Fig. 3.—Ova of *P. caucasica* (A) and *P. mordens* (B), of the same magnification.

Fig. 4.—Ventral aspect of posterior extremity of a male *P. mordens*, showing the arrangement of the papillæ.

### Explanation of Lettering in the Figures.

c.—Cuticular ring embracing the two lips posteriorly.

c. b.—Cuticular bosses guarding the mouth laterally.

ch. i.—Chyle intestine.

B.—Bursa.

ej. d.—Ejaculatory duct.

e. d.—External tooth.

i. d.—Internal tooth.

ped. p.—Pedunculated papillæ.

ses. p.—Sessile papillæ.

sm. p.—Submedian papillæ.

sp. 1.—Long spicule.

sp. 2.—Short spicule.

t. c.—Testicle coils.

ves. sem.—Vesicula seminalis.

equivalent to about a fourth of the entire body length. Beyond the anterior limit of the vesicula seminalis the testicular tubule continues forwards with two or three longitudinal, but practically no obvious, lateral folds to within a distance (about equal to the length of the ejaculatory duct) of the œsophagus where it returns upon itself, the fine terminal part of the testicular tubule being found at about the centre of the body. The spicules are very unequal, the larger and more slender measuring 4·6 mm., the shorter and stouter ·6 mm. In the female, the detailed arrangement of the two uterine tubules is not so easy to be followed, owing to dilatation of these organs with ripe ova. It can be seen, however, that the common conducting tube formed by the union of the two tubules opens by a transverse slit on the ventral surface of the body in the region between the juncture of the anterior fifth and the anterior fourth of the body length with the remaining portion.

The ovarian portions of these tubules are found greatly coiled with one another at the posterior part of the body cavity just in front of the rectum; the actual ends of the tubules are seen to be somewhat dilated and club-shaped.

The mature eggs have a thick smooth cuticular shell, and are much smaller than those of *P. caucasica*. They measure ·043-6 by ·035-3 mm. (Fig. 3).

The characters upon which the specific identity of the species may be established are: The large size of the adults; the peculiar construction of the lips and the simple formation of the inner tooth, the anterior edge of which is undivided. In the male, the disposition of the papillæ; the internal arrangement of the testicular coils; the size of the spicules. In the female, the size of the ova, the position of the vulvar aperture, and the sharpened posterior extremity of the body.

As already stated, the characters given for *P. caucasica* are few in number. The description of the mouth parts contains only facts of generic importance. Its small size, the different relation of its papillæ to one another, and the difference in size of its spicules and ova will suffice to distinguish it from *P. mordens*.

The type-specimens are deposited in the Helmintholo-gical Museum Collection (Human Series, No. 30) of the London School of Tropical Medicine.

## MEETING HELD AT 20, HANOVER SQUARE, ON FRIDAY, JANUARY 17TH, 1908.

SIR PATRICK MANSON, K.C.M.G., President of the Society, in the Chair.

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THE Minutes of the last meeting were read and confirmed.

The PRESIDENT announced that Dr. Johnson, who at the November meeting exhibited in his own person an excellent example of Oriental sore, had kindly consented to be present that evening, in order to show the result of Mr. Cantlie's treatment by scraping two of the sores as compared with spontaneous cure in the unscraped sore. He desired to mention as an interesting fact in connection with the case that Oriental sore was not altogether unknown in this country, because that afternoon he had seen a little child, aged four, whose mother told him that about a year ago, while in India, the boy had been bitten by mosquitoes, and as a consequence of the mosquito bite a sore of a chronic character had developed under the chin, which after a year was still unhealed. He had scraped the sore, and found the Leishman body, showing that it was a true Oriental sore.

# THE UNEQUAL DISTRIBUTION OF FILARIASIS IN THE TROPICS.

By G. C. LOW, M.B.

(*Friday, January 17th*, 1908.)

The distribution of any given disease is always a very interesting problem, and one which of necessity must depend on many varying factors. This is specially so in many tropical complaints, and, such being the case, I have hoped by introducing one of those to your attention to-night to stimulate further work and research in the whole question of the unequal distribution of disease throughout the Tropics. Most of the well-known tropical diseases are peculiar in this respect, namely, that they require an intermediary either in the shape of an animal or an insect to further their proper dissemination, and if, theoretically, we could remove those, then the diseases would necessarily come to an end. Examples of those are plague, with its dependency on the rat and its fleas; Malta fever, with its dependency on the goat; sleeping sickness conveyed from man to man directly and apparently only by tsetse flies; malaria, yellow fever, and filariasis, requiring something more complicated still, namely, a definite extra-corporeal life cycle in different genera of mosquitoes; and so on.

The dictum, "No anopheles, no malaria," is easy of understanding, and examples are not far to seek. Barbados, an island in an intensely malarial zone, is free from malaria, and the reason of this I showed some years ago was due to anopheles mosquitoes being absent. Rome is now in the same category, and other Pacific islands are said also to be free, the danger of the chance introduction of anopheles being exemplified by the present-day condition

of Mauritius. If we extend such a dictum further we may say, "No *Stegomyia fasciata*, no yellow fever," and "No *Culex fatigans* or other suitable mosquito, no *Filaria nocturna*." What is slightly more difficult to understand is, why a disease should remain localised to certain parts of the world, although its intermediate host is much more widely distributed? Why, for example, should yellow fever have remained so particularly endemic to the West Indies and South America when its intermediate host, *S. fasciata*, is found throughout the Tropics? It is possible that non-introduction of the specific germ is the cause, but behind this may there not be something deeper that we do not yet thoroughly appreciate? Lastly, what is even more difficult to understand is why, given the intermediate host of a disease, suitable climatic conditions, and the presence of abundant chance of the further introduction of the specific parasite, the disease should vary considerably in its distribution, abounding in one place, being scanty in another, or practically absent in a third? *F. bancrofti* and the diseases it causes follow such an irregular course, and it is on this interesting feature that I intend to base my paper to-night.

In dealing with filariasis (including, under this term, the parasites *F. nocturna*, *F. demarquayi*, and *F. perstans*), I have probably taken the most difficult of all the tropical diseases because of the tediousness in determining its exact distribution. I may remind you that it is the exception for people infected with *F. bancrofti* to exhibit symptoms, so the only way to arrive at a conclusion of how many individuals in a given district are infected is to make exhaustive night blood examinations of the population generally, the same procedure being adopted with the exception that the blood may be taken by day for *F. demarquayi* and *F. perstans*. This, as those latter two parasites produce no appreciable clinical signs, you will see is imperative. A group of islands is a suitable area to

conduct such observations in for many reasons: they are isolated from each other, many of the inhabitants have never been out of them, the population is not excessive, and, as a rule, there is one main town with several villages. Recently, then, when in the West Indies I worked out the percentages of infection with *F. nocturna* and *F. demarquayi* in many of the different islands, and have tabulated some very interesting results, which, on the whole, may be taken as approximately accurate.

My plan of procedure in this work was as follows:—On arrival at an island I inquired from the local medical men if clinical manifestations of filariasis (elephantiasis, lymph scrotum, chyluria, lymphangitis, etc.) were common; then after looking for them myself generally, but of course without specially selecting them for my statistics, I took the night blood from as many of the general population as I could get, obtaining these from the streets, hospitals, prisons, and almshouses, and trying so far as possible to take indigenous natives. In some of the places visited I separated the different races, as this is important, but in others the admixture was so great that this was impossible. Out of the numbers examined I noted how many showed signs of filarial disease, or were infected, namely those with symptoms with or without embryos, and those without symptoms, but with embryos in the blood. For *F. demarquayi* I examined the different villages in detail as well, sometimes taking the blood by day, sometimes by night, and as those embryos are present at both times, night examinations did both for them and nocturna. The results came out in the following manner: Where there was much clinical filarial disease, elephantiasis, etc., then the percentage of ordinary healthy people with embryos in their blood was high; where there was little disease, then the percentage was low. Applying such tests to the different islands and British Guiana, all, with the

exception of the latter and Barbados, much the same physically, and all, Barbados and British Guiana included, teeming with the proper intermediate host (*C. fatigans*) for *F. nocturna*, the inequality of the distribution was very marked, reaching a very high percentage in St. Kitts, and falling to practically zero in Grenada. The following are the detailed results in order of frequency:—

*St. Kitts.*—143 cases examined by night; 47 suffering from *F. nocturna* disease = 32.8 per cent.; with symptoms 9, without 38. White people of the best class: 23 examined, 7 infected = 30.4 per cent.; with symptoms 3, without 4. Elephantiasis cases: 14 examined, embryos present in 3.

*British Guiana.*—150 examined by night; 25 suffering from *F. nocturna* = 16.6 per cent.; with symptoms 5, without 20. Race characteristics: general population of hospital, negroes, coloured, Portuguese, and whites; 100 examined, 22 infected = 22 per cent.; with symptoms 5, without 17. Coolies (East Indians), 50 examined; 3 infected = 6 per cent.; with symptoms *nil*, without 3. Elephantiasis cases: 15 examined, embryos present in 1.

*Barbados.*—600 cases examined by night; 76 suffering from *F. nocturna* = 12.66 per cent.; with symptoms 27, without 49. Elephantiasis cases: 10 examined, embryos present in none.

*Race Characteristics.*

| Race. | Number Examined. | Non-infected. | Infected. | Percentage. |
|---|---|---|---|---|
| Negroes ... | 401 | 357 | 44 | 10.9 |
| Mulattos ... | 160 | 142 | 18 | 11.2 |
| Whites .. ... | 39 | 25 | 14 | 35.3 |
| Total ... | 600 | 524 | 76 | 12.66 |

*Trinidad.*—400 cases examined by night; 43 suffering from *F. nocturna* = 10.75 per cent.; with symptoms 19; without 24. Elephantiasis cases: 83 examined, embryos present in none.

*Dominica.*—144 cases examined by night; 11 suffering from *F. nocturna* = 7.63 per cent.; with symptoms 2, without 9. Elephantiasis cases: 2 examined, embryos present in neither.

*St. Lucia.*—356 cases examined by night; 27 suffering from *F. nocturna* = 7.58 per cent. Elephantiasis cases: 5 examined, embryos present in none.

*St. Vincent.*—100 cases examined by night; 6 suffering from *F. nocturna* = 6 per cent.; with symptoms 2, without 4. Elephantiasis cases: 2 examined, embryos present in neither.

*Grenada.*—174 cases examined by night; none infected = 0 per cent.

*Carriacou* (a Grenadine).—28 cases examined by night; 1 suffering from *F. nocturna*, but this individual was born and had lived in St. Kitts, only having been in Carriacou one year; so deleting him we get a percentage of 0.

Looking into those figures in detail, one notices a very great variability in the amount of filarial disease in the different islands, St. Kitts, one of the smaller islands, heading the list; British Guiana, which, of course, is not an island, but on the mainland of South America, coming next; Barbados and Trinidad, both densely populated, next; with the smaller islands, Dominica, St. Lucia, St. Vincent, and Grenada last. Why this should be is not very clear, and the case of Grenada is so peculiar that I quote the note I made on it on leaving the island :—" Filarial (*F. nocturna*) disease is very rare in Grenada, this answering to the clinical experience of the medical men in the island, who state that they very rarely see cases. It does, however, exist, and can, as *C. fatigans* is present. This is exempli-

fied by one case I saw, a man who, though never out of the island, has varicose groin glands and elephantiasis. (Not being able to get his night blood, I did not include him in my general list.) Similar other rare cases are to be found, two such having been heard of. In addition to those examined, I also saw one Barbadian with elephantiasis, and another with filarial embryos in his blood. The marked exemption of the island may be due to the chief town being mainly on a hillside, and to there being few mosquitoes of the species *C. fatigans* present." The medical man at the small village at Carriacou (an island to the north of Grenada) also informed me that he never saw filarial cases there, and my figures obtained bear out his statement. During the day and night I spent there I saw one typical case of elephantiasis, however, but he had lived in Trinidad, where he probably got his infection, and the individual in whose blood I found embryos as already mentioned had without doubt acquired those in St. Kitts. In my note on Grenada I mentioned there being few mosquitoes (*C. fatigans*) present, but still I found examples without much difficulty, and they were certainly very abundant in St. Lucia, which is also only slightly infected. It is probable that the number of mosquitoes present may have something to do with the prevalence of the disease, St. Kitts, for example, owing to the dirty privy system in vogue there, simply teeming with *C. fatigans*, and they also are very numerous in Barbados; but, considering all things, I have come to the conclusion that there is something more subtle at work than this in determining the distribution of this disease. Climate, by that meaning temperature, rainfall, etc., cannot play much part, as it is practically the same in all the islands, and the same might be said of physical characters, though at the same time I think the flat, low-lying, and thickly-populated places are specially prone to the disease. Barbados is of coral formation, the other

islands volcanic in origin. Looking at other parts of the world, I have found in the Swahilis (coast natives) of Mombasa thirteen out of fifty infected with *F. nocturna*, none of these showing any symptoms, and this giving a percentage of 26. In Zanzibar it is probably quite as prevalent, because, though not making blood examinations there, I nevertheless saw plenty of cases with definite symptoms. In Uganda, in the interior, it is practically non-existent in the indigenous races, as I only found it in one pure Waganda, though in Nubians from the Nile it occurred in 2 per cent., and I also had a well-marked case of elephantiasis in a Nubian woman. Its absence from the interior might probably be due to the absence or scarcity of *C. fatigans*; but then, again, *Mansonia africana*, which Dr. C. W. Daniels easily infected with *F. nocturna* experimentally in British Central Africa, abounds in Uganda also. Quoting from the book of our President, Sir Patrick Manson, on the Prevalence of *F. nocturna*, I note South China given as 10 per cent.; Friendly Islands, 32 per cent. (Thorpe); Samoa, very high; Madras and West Africa, abundant. In Egypt it is present (Sonsino and others), but I do not know if its percentage has ever been accurately determined. Perhaps Dr. F. M. Sandwith may help us in this. In Brisbane, in Australia (Bancroft), originally introduced by the Chinese, it seems to have flourished and spread, and it also occurs in Charlestown, in North America.

In dealing with the distribution of *F. demarquayi* and *F. perstans* we are not on so certain ground, as we do not know the proper intermediate hosts of those parasites yet, unless the tick, the *Ornithodoros moubata*, as Wellman believes, acts for the latter. This being so, their irregular distribution may depend on the presence or absence of the proper intermediary; but whether this is the reason or not, I can certainly bring forward some remarkable points as

regards their prevalence in certain places. *F. demarquayi* was discovered by Sir Patrick Manson in blood slides sent to him from the West Indies, from St. Lucia and St. Vincent by the late Dr. Otho Galgey and Dr. Newsome respectively, and he gave it the above name in honour of Demarquay, who first discovered the embryos of *F. nocturna*. On arriving at St. Lucia, Dr. Galgey informed me that the slides he had sent Sir Patrick Manson were taken from one of the villages of St. Lucia, Gros Islet by name, and that he believed most of the cases of this parasite were to be found there. Accordingly I investigated the subject, and found that the belief, with one possible exception, was perfectly correct, the parasite was limited to this one village of the island. St. Lucia has one main town, Castries, and five subsidiary townships or villages, namely, Sufrière, Vieux-Fort, Dennery, Gros Islet, and Anse-la-Raye. On examining the blood of people from these different villages, I got the following results :—Gros Islet : 62 people examined ; 16, or 25.8 per cent., infected with *F. demarquayi*. Sufrière : 78 people examined ; 2 infected, those two, however, being policemen who had lived for periods in Gros Islet. Anse-la-Raye, none. Vieux-Fort, none. Dennery : not examined. Castries : 3 infected, two of those Barbadians who had lived in different parts of the island, Gros Islet included ; the third a negro, aged 19, with the following record :—He was born and had lived in Charlestown, Nevis, till 17 years of age, then went to Castries, then working in the fields at a sugar estate, called Roseau, for four months, then back to Castries with fever ; has remained in the town since, never in Gros Islet or north of island. It is possible he may have been infected at Nevis before going to St. Lucia. I also saw the infection in a man from a place called Monchy, near Gros Islet, and in another man who had lived all over the island. The one possible exception was Dennery, a small

village on the north coast which I did not examine, and it is just possible that *F. demarquayi* might exist there, though Dr. Galgey had never found it in a few examinations he made from there.

In St. Vincent the same striking peculiarity of the parasite being practically localised to one spot occurred. St. Vincent, like St. Lucia, has one main town, Kingstown, and several villages—viz., Calliaqua Layou, Barrouallie, Georgetown, and Chateaubelair. I examined blood from all those places with the following results: Calliaqua, 50 examined; 16 infected = 32 per cent. The others were all uniformly negative, with the exception of 1 case from Barrouallie who had lived in different parts of the island, Calliaqua included, and another discovered in Kingstown who had also lived at one period in the infected village. Having had the experience of St. Lucia before me, and suspecting that the same might hold good in St. Vincent, my procedure, on arrival at the latter place, was as follows: I asked Dr. Newsome where he got the blood he sent to London from, and he gave me the name of a woman living in Calliaqua. I thereupon visited her (September 21st, 1901), and found her suffering from malarial fever, counting at the same time, however, 40 embryos of *F. demarquayi* in her blood, and making the following note in my record-book: "Embryos of *F. demarquayi* found in London in 1894 by Dr. Manson." Other inhabitants of the same village examined at the same time showed similar embryos, and the infected place being found, all that remained was to see if any other parts of the island were infected. In addition to St. Lucia and St. Vincent, I also determined the presence of *F. demarquayi* in Dominica, Trinidad, and probably in St. Kitts, but was unable, owing to difficulties of transport and time, to find the exact focus in each place. In Dominica 2 cases were found, 1 in an old soldier who had served on the

West Coast of Africa and had been in the other islands, so he might have got the infection elsewhere; but the other, a mulatto born and living in a place called Batalie, had never been out of the island. He suffered from elephantiasis as well—*e.g.*, a double infection—but had no nocturna embryos in his blood. In Trinidad 2 cases were also found, 1 in a negro born in Dominica who had also lived in St. Vincent and St. Lucia, he therefore being useless; but the other, a case of Dr. Vincent's, a girl in the lunatic asylum, born at Matalot, a village in the north of the island, had never been out of the island. No further bloods were obtainable from that place, but the 1 case is sufficient to prove that the parasite is found in Trinidad in the north, though I never found it in the west and south. In St. Kitts, again 2 cases were found, 1 in a man with elephantiasis of the scrotum, a sailor, who had been in Dominica and the other islands; the other in a Portuguese, born in Madeira, who came to St. Kitts when a youth, and had only once been out of the island, when he lived in Port of Spain for a short time. The rest of his life had been spent in Cayon, a village on the windward coast of St. Kitts, so this probably means a focus of infection there.

Looking at the infected villages in St. Lucia and St. Vincent from a physical point of view, there was nothing special to be noted in them as different from the other non-infected places. They were both situated on the sea coast on level ground, with swamps and scrubby bush behind them, and were very malarious. Assuming that a mosquito or other blood insect might be the intermediary, many experimental feedings were made with negative results; the only out-of-the-way mosquito noted was at Calliaqua, where the inhabitants told me that a big bright blue one sometimes was seen. This was the *Hæmogogus cyaneus* of Williston; but I could only obtain one, and

it died before I could feed it on a suitable case. Since Wellman's work on the tick as the spreader of *F. perstans* appeared I have wondered if such an insect could act as an intermediary for *F. demarquayi*, and this is possible, though even were this so, it is strange how it should be so closely limited to one part of the island, and not spread universally. Still, ticks of different sorts are very common in the West Indies and the forests of British Guiana, and the hypothesis is worth following. It might even be a bug-like tick like the ornithodoros of Africa. *F. demarquayi* is not found in Barbados, and I never met with it in Grenada. In British Guiana two embryos are often found in the blood of aboriginal Indians and others living in the backwoods of that colony. One is blunt-tailed, and is *F. perstans*, or a closely-allied species; the other is sharp-tailed, and may be *F. demarquayi*, though Daniels, in comparing the adults, believes there are differences, so it may be another species. Those embryos are not found in the blood of people inhabiting the towns or cleared coast lines, only in the forests. The following are statistics on their prevalence:—Aboriginal Indians (Arawaks, Caribs, Wainis, Waraus, Akawoios, etc.), 163 examined; 105 infected with sharp or blunt-tailed embryos = 64.4 per cent. Double infections, 38; blunt-tailed alone, 56; sharp-tailed alone, 11. Half Indians (*e.g.*, mixtures of Spanish, Portuguese, etc.), 20 examined; 5 infected = 25 per cent. Creoles, (*e.g.*, negroes, mulattos, and whites who had lived in the interior), 28 examined; 3, all negroes from the Pomeroon River, only infected. Districts: Pomeroon River, 71 per cent. infected; Cara Cara Creek, Demerara River, 60 per cent. infected; Barima River (Morawhana District), 36.6 per cent. infected; Waini River, 60 per cent. infected. Those figures show that the infection is widespread and pretty equally distributed in the districts examined.

In Africa blunt-tailed embryos (*F. perstans*) abound in

some districts, while they are entirely absent from others, and no one has yet described sharp-tailed embryos on that continent corresponding to the New World forms. What circumscribes their distribution is again difficult to decide; the absence of the suitable intermediate host, if they have to pass through one, would of course preclude them; but probably again other factors play a part. Wellman has lately brought forward some strong evidence that they can undergo a metamorphosis in ticks (*O. moubata*), and their distribution seems to coincide, except apparently in British Central Africa, where, though the tick is very common, Daniels in some examinations on natives there found no *F. perstans*. In the vicinity of the equator, Uganda, and the Congo the parasites are very frequent in the blood of the natives there, in some districts almost everyone having them. The following figures represent such infections in some of the parts I have studied them:—Zanzibar and Mombasa, negative; Kavirondo, at the east of the Victoria Nyanza, 0 per cent; Wagandas in Entebbe, 50 per cent. infected; natives of the Sese Islands in the north-west corner of the lake, 86 per cent. infected; natives of Ankole, to the west of the lake, 7.8 per cent. infected; Alurs from Wadelai (Dr. A. W. G. Bagshawe), 8.4 per cent. infected; Nubians from Upper Nile, 6 per cent. infected. From this it will be seen that the Sese Islands in Uganda are a strong focus of infection, the prevalence decreasing to the west, the north, and entirely disappearing by the time the north-eastern shores of the lake are reached. On the western side of the continent it also prevails in many different parts. Mr. T. E. Rice, at Ibadon in Lagos, found 30 per cent. of the natives infected there. Dr. St. George Gray has informed me that it is fairly common in the hinterlands of Sierra Leone, Dr. Wellman reports it at Benguella, and I have seen it in slides from the Congo and the Nigerias.

A good deal more mapping out and general search for those latter two filariæ in different parts of the world is still required. There may quite conceivably be other species undiscovered—for example, is the sharp-tailed one from New Guinea described by Sir Patrick Manson the same as *F. demarquayi*? The Malay States, Borneo, and Sumatra have not yet been tapped, to quote only a few places. It is the tediousness of searching blood slide after blood slide for filariæ that keeps men from working on this subject, and then the further trouble of isolating villages and working them out in detail often adds insurmountable difficulties. Still, I think you will agree with me that, after the figures I have collected and given you to-night, the subject of the distribution of all the three filariæ is extremely interesting, and if we merely limit ourselves to *F. nocturna* alone, which, after all, is the only one that produces disease, we will still have abundant food for thought and abundant stimulus for research upon the unknown influences which allow it to spread in one place while limiting it in another. By focussing our attention on such a subject, we may also indirectly lay bare some of the secrets that surround malaria, yellow fever, beri-beri, and many of the other Tropical diseases, and by so doing may open up new fields for the prevention and better control of those scourges of mankind.

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## Discussion.

Dr. Sambon, in opening the discussion, thanked the author for his exceedingly valuable paper, especially for the data which it contained. Some years ago he read a paper at the Epidemiological Society on the subject, in which he showed that disease carriers were certainly concerned, and were the important factors in the geographical distribution of disease. That fact explained why certain

diseases were limited in their distribution and why others were widely spread. With regard to filaria, he thought the different species of filariæ varied greatly in geographical distribution. *Filaria bancrofti*, for instance, which seemed to be almost world-wide in its distribution, might, he thought, be found to be by no means ubiquitous, because he believed that under that name several worms had been confounded. Looss had already pointed out that there were material divergences in the descriptions of various filariæ from different parts of the world, and this was particularly true of the anatomical details of *F. bancrofti*. The uterine tubes, for example, were described as showing many structural distinctions. Then the description Thorpe gave of the larval forms of filaria observed by him did not correspond with the larval forms of *F. bancrofti* as we know them. It was evident that previous observations were incorrect, or that, as was probably the case, another filaria had been under investigation. With regard to the limitation or absence in particular places of certain filariæ, for instance, *F. bancrofti*, when it might have been expected to be plentiful because of the abundance of its appropriate carrier, it was a fact that, not infrequently, as in the analogous case of malaria, *F. bancrofti* was absent. In India, Italy, and France there were spots, sometimes in the heart of an intensely malarial region, in which malaria was unknown. Possibly the reason why malaria and *F. bancrofti* were absent in those places was due, as had already been suggested, to hyperparasitism. Thus it was now known that the brown spores or "black bodies of Ross," which that investigator believed might represent a stage in the life-history of the malarial plasmodium, were protozoal organisms—a species of nosema, parasitic on the malarial parasites, and Noya, in Italy, had shown that the same kind of thing occurred in other protozoal infections. It was quite possible that hyperparasitism might also be

an explanation of limitations in the distribution of *F. bancrofti*, and that it, too, was destroyed by a secondary parasite. Dr. Low had mentioned that Wellman suggested that the carrier of *F. perstans* was *Ornithodorus moubata*, but Feltmann, before Wellman, had suggested *O. moubata* as the possible carrier of *F. perstans*. He did not know whether Wellman was considered as having proved his point; but, in any case, if it was so, it would be difficult to explain in what way *F. perstans* was carried to places where *O. moubata* was not found, and where the infection undoubtedly prevailed. In America there was a species of argas which might take the place of *O. moubata*, namely, *Argas americanus*, and it was known that the spirochætes of African relapsing fever were carried by it as well as by *O. moubata*. In Persia, again, there was another disease, most probably akin to relapsing fever, which was carried by another tick, viz., *A. persicus*, a tick which, much in the same way as *O. moubata* in Africa, infested the houses of the people there, and apparently had a similar life-history. In America, still another tick—*A. miniatus*—was the common representative of the family, and it, no doubt, took in that country the part that *O. moubata* played in Africa.

Dr. Leiper stated that Dr. Low had referred to the prevalence of *F. perstans* (*i.e.*, the blunt-tailed embryos) in the blood of African natives and to the absence of the sharp-tailed embryos (*F. diurna* and *nocturna*), and contrasted that with the condition of distribution which he found in the West Indies. On a recent expedition he (the speaker) had an opportunity of visiting East Africa and Uganda, and he was able to confirm, while in Uganda, the observations that Dr. Low has made. With regard to the prevalence of *F. perstans* in the blood of natives round Entebbe, it was the case that about 50 per cent. of them

were infected, and this condition prevailed right up to the Murchison Falls, at the north end of Lake Albert. While it was true, however, that sharp-tailed embryos did not occur in Africa in man, it was scarcely true that they were entirely absent in that continent, because sharp-tailed embryos were found in the blood of African monkeys, which resembled, in a very marked degree, the embryos of *F. nocturna* found in man in the West Indies. The resemblance was indeed so great that it was impossible without considerable study, to detect any difference. He had recently had an opportunity of corroborating that infection of monkeys at the Zoological Society's Gardens in London. He desired, too, to lay stress on a point to which Dr. Sambon had referred, namely, that a variety of helminths might be included under what was considered to be a single species when only larval forms were examined. As Dr. Sambon pointed out, it might, for instance, be found that two or three species had been classed as *F. bancrofti*. This could only be ascertained when the adult forms were examined with more detail. A much wider examination was necessary in the case of filarial embryos also, for he believed that would show that the characters by which they differentiated the various species of larval forms would no longer be found to hold good. As a matter of fact, they could not depend on larval forms only to determine distribution. The question of sharp-tailed embryos in Africa was to some extent solved by his discovery in the retro-peritoneal tissue of monkeys at the Zoo of adults quite different from what one would expect to find if the sharp-tailed specimens had been embryos of *F. demarquayi*; in fact, on looking up the literature, he found that a similar species had been described as *F. rossi*, and that it had already been described elsewhere as *F. coronodes*. It was not possible, by looking only at the embryo, to say it was *F. coronodes* and not *F. demarquayi*; and he

thought that emphasised the necessity of being very careful in drawing conclusions as to species from an examination of the blood parasites only. The distribution of filaria in different parts of the world could only be determined by careful comparison of adult parental forms, and by an equally careful examination of embryos found in the blood.

Dr. F. M. Sandwith said that Dr. Low had kindly thrown out an unexpected but friendly challenge to him to give some information about filariasis in Egypt. He had nothing to say which would in any way help so carefully thought out a paper as they had just heard, but he might say that filariasis was not an extremely common disease in Lower Egypt. When Dr. Low spoke of Nubians coming from the Nile, he meant from the point of view of Uganda or Central Africa, that they came from a relatively northern latitude. But when one was in Egypt one looked upon Nubia as a very southern latitude. As a matter of fact, in Egypt that part was called the Egyptian Soudan. How far the disease existed among the Nubians he did not know, and if Dr. Low did not know, he did not know anyone else who did. He (the speaker) went to Egypt rather more than a quarter of a century ago, when there was only one man in the country working on the subject of worms, namely, Sonsino, to whom Dr. Low had referred. Sonsino had made many observations on filariasis, and had discovered many things in his own generation, and he (Dr. Sandwith) went to him when he required information about that special disease. Sonsino corroborated the little he had learned, and he found that most of the cases in Cairo, where he was then stationed, had come from Damietta and Rosetta, two seaports, roughly in the latitude of Alexandria. Among the out-patients in the hospital at Cairo, they occasionally met with people with elephantiasis

of the arm, leg, mamma, or some other part of the body, very commonly the scrotum. They began to wonder why it was that the disease was produced in those two areas, chiefly in these two towns, and in Alexandria to a certain extent also. It was then found that the inhabitants of Damietta and Rosetta lived almost below sea level, at least below the river level where the river ran into the sea, and that they were obliged, for certain months in the year, to use an artificial water supply. Instead of getting their water from canals as the remainder of the people of Egypt did, they had to store their water in reservoirs and cisterns. That was necessary, because during the low Nile season the sea came in, and as the water supply was below the sea level it then became so salt that the people could not drink it. Therefore, for generations they had found it necessary for some months in the year to have cisterns to store fresh water. He supposed Dr. Low would say that it was from drinking that water that the people got elephantiasis; that the cisterns supplied excellent breeding places for *Culex fatigans*. Anyhow, on that supposition the people were told not to drink the water from the cisterns, and a better water supply was introduced. The cisterns were abolished, and also the elephantiasis to a considerable extent, but not entirely; it was, however, now a comparatively rare disease. The next point to which he wished to refer was that in cases of elephantiasis they did not observe the *F. nocturna;* they shared a general experience in that they could not find filarial embryos in the blood of patients who were obviously suffering from elephantiasis. But he had come across a case of chyluria in the out-patient room, in which filaria embryos swarmed in the blood, and they would gather how rare the disease was in Egypt when he told them that he had to pay that patient a yearly retainer to consent to remain under observation, to agree to be bitten by mosquitoes, and to be shown off, as it were, to each

succeeding generation of students. It was not the only case he saw, there were a few other instances, but it was the only case he knew he could lay his hands on at practically a moment's notice. Filariasis in Egypt was almost as rare as leprosy in this country. When he became somewhat more instructed as to the various kinds of filaria, he spent many midnight hours hunting for embryos because their President, whose power of stimulating others to work was so well known, always said he was certain there was plenty of filariasis in Egypt, and practically suggested that it was his (the speaker's) fault that the embryos had not been found. He would not say that he never found them, but he very seldom found them, and he was therefore prepared to say they were by no means common in Cairo. In the hospital there a great number of patients came from Lower Egypt, and when they came from Damietta and Rosetta one naturally expected to find the disease, and did so. However, he had known many investigators who had given up considerable portions of many nights to research say that the game was not worth the candle, and that they found so little to repay them that they preferred to spend their time in a more useful way. But it was only right to confess that there was one exception to that experience. Dr. Hayward, who was now at Port Said, had once examined blood films from the whole of the 400 patients in the Kasr-el-Ainy hospital at Cairo and found that 15 per cent. of them were infected; and it must be admitted that that rather looked as if an intelligent worker might succeed in finding more than others less practised in technique had found before him.

The President, personally, as well as on behalf of the Society, thanked Dr. Low for an admirable paper, one which he thought would undoubtedly be of great use in directing and stimulating further investigation into a very important

department of what he might designate filariology, viz., the principles regulating the distribution of filaria. They had found the means by which the germ was transmitted and acquired, and they were apt to think that the subject was finished, but the reverse was the case. The more that was learned about any disease the more required to be known. That was especially true of filaria. Why should it be that in countries where *C. fatigans* was equally prevalent the disease it produced was very common and in another it was very rare? That question was difficult to answer, but that there was an answer to it was undoubtedly the case could the discovery but be made. He believed that if a satisfactory reply could be given they would be at once placed in possession of the means to counteract the pathogenic influence of the filaria. It had always appeared strange to him that, when natives of an infected country became inoculated with filaria, they did not get devoured, like Herod of old, by their own worms. In considering the case, say, of a coolie who was infected with filaria, it must be remembered that he lived in the same miserable tumble-down hut year after year, and that he was in consequence bitten by the same mosquitoes. Every mosquito that bit him became infected, and after it had become infected the filaria developed, and the mosquito again bit, and again infected him. This reinfection process went on day after day, month after month, and year after year, until one might think that in time the man would become a mere helminthological museum, his body nothing but a collection of worms, and yet that was not the case. An extreme degree of filariasis in any individual was a rare thing. In every film of blood, 10, 15, 20, or even more filariæ could be found, but there was always a limit. What was that? What was it that restricted the propagation of the filaria? Dr. Sambon suggested that it was hyperparasitism, and threw out the idea that that might be due to some special

form of secondary parasite. Personally he had never seen hyperparasitism in the case of *F. nocturna*. He had fed hundreds of mosquitoes on filarial subjects, and had never failed to succeed, when he took the proper precautions, in getting the filaria to pass through the whole of its developmental cycle, and he had never seen the filaria become diseased in the mosquito. There must be some other explanation. Another interesting and very important problem in connection with filariasis was to determine the circumstances which caused it to produce disease. There was no question about filariasis being the cause of chyluria, of lymph scrotum, and similar troubles. There was a little doubt as to its being the cause of elephantiasis, but he considered the fact to which Dr. Low had given prominence, the fact that in a country where filariasis was prevalent patients who were the subjects of elephantiasis were rarely affected, or all events seldom showed embryos of the parasite in their blood; that, he thought, was a strong proof that the parasite was the cause of the disease; were it not so, why did not they harbour embryos in the same ratio as the rest of the population? Why should the parasite be absent only in these cases? Something he believed had happened to the subjects of elephantiasis previously, when they were the subjects of active filarial infection, which set up the disease and brought about the death of the parasite. The exact process was for the future to determine, but it was his considered opinion that elephantiasis originated in filaria-caused obstruction of the lymphatics; but whether that was a correct statement of pathological fact or not no man could at present say. It was a subject which required active study by capable investigators working in the endemic zone, and he strongly commended it to anyone who had the opportunity. Allusion had been made by Dr. Leiper and others to the difficulty of determining species of nematodes by examina-

tion of the embryos only. He quite agreed. It was his impression that there were not only three or four nematode parasites with blood-haunting embryos, probably there were at least twice that number; but, as a rule, they saw the embryos only, and the difficulty was to identify species by it alone. He thought it was possible that they had been working all that time with several species of *F. nocturna*, and he had a sneaking idea that perhaps some of his own work had been done under false names, and that some of the worms were not exactly what he supposed them to be; but that also was for the future to show. Until the mature worms were obtained it was impossible, in many instances, to determine from the embryo the species of the parasite.

Dr. Low, in reply, said the subject of the paper was a most difficult one, one of the greatest difficulties being, as Dr. Leiper said, that of identifying species from embryos or larval forms alone. It must be remembered, however, that a certain amount of reliable work could be done by careful measurements. For instance, he was pretty well certain, after examining the sharp-tailed embryos in the African monkey to which Dr. Leiper had referred, that they differed from *F. demarquayi* in some of the minuter measurements. The adults found in the monkeys at the Zoological Gardens were very different. It was an extraordinary thing that two totally dissimilar adults might produce an almost identical embryo. For example, the parent forms, of *F. bancrofti* and *F. loa*, were entirely different, and yet they produced embryos which, even by the initiated, might be mistaken the one for the other. When there was such a similarity in the larval forms produced by such dissimilar adults, difficulty must arise, and to be absolutely certain of species it was necessary to see the mature worms. *Post mortems* were not easy to get,

and even when an opportunity arose from making a *post mortem* it meant a hunt of several hours' duration, for adults were most elusive. He was convinced that there was something beyond their present understanding to explain the diverse distribution of filariasis. He agreed with Sir Patrick's remarks with regard to hyperparasitism. He had not seen it himself in the large number of feeding experiments he conducted. If the proper mosquitoes were used, they would go through the process time after time, although the results might vary a little with the weather and other climatic influences. He did not think, therefore, that hyperparasitism could be a factor in making the disease rare in one case and abundant in another. He had carefully studied elephantiasis cases in filarial countries, principally because it was disputed whether elephantiasis was a manifestation of filarial disease. As a result, he was perfectly convinced that it was so. In the West Indies, especially in Barbados, he had traced the production of elephantiasis from its very commencement; and, as there was no malaria in Barbados, it was possible to study filarial cases in what might be called pure culture. The first general manifestation of trouble was the so-called fever and ague—a wrong term, but nevertheless a term which described a definite clinical entity. Its synonym was filarial lymphangitis, and subjects of filarial infection were very prone to this form of inflammation. If people liable to it bathed in the sea when they had, say, a trifling scratch on the leg, an attack of lymphangitis would be excited. The disease it simulated most was erysipelas; there was a swelling of the leg; in the case of white people, a red blush was seen; there was great enlargement of lymphatic glands in the femoral and inguinal regions, and they were tender; the patient was often delirious; he vomited and had a high temperature, mostly preceded by rigor. There might be resolution, when the leg would become

quite normal. Such a case, however, might get repeated attacks, and on following those up he found that, after each illness, there was some little added thickening left; the thickening went on increasing with each seizure, the lymphatics were more and more infiltrated by inflammatory products, and they ultimately passed into a condition of true elephantiasis. The disbeliever might object that these cases were certainly lymphangitis, but there was no proof that they were filarial. The curious thing was, that if the blood was examined after even a first attack of lymphangitis, embryos could not be found in the blood; he had examined large numbers of pure cases of filarial lymphangitis, and had never found embryos in the blood. The disbeliever might again, therefore, say that showed that filaria had nothing to do with it; but personally he believed it had, and, more, that it was actually the cause of the lymphangitis. He thought it was probable that the death of the parent plus streptococcus was the real origin of elephantiasis. Yet once more it might be said that if the filaria died thus in the beginning, why should there be persistent and repeated attacks? It could not be from the death of a new parent filaria every time. That was a problem which only many *post mortems* could solve, and, as he had said, they were difficult to get; but before any large advance could be made in our knowledge of this branch of pathology, they were necessary. With regard to heavily-infected localities, in his figures, he mentioned three cases of elephantiasis with embryos in the blood in St. Kitts. He ascribed that to the fact that probably there was such a high index of infection there that the cases were really reinfections. The first parent filariæ must have been dead years before, but living in such a heavily-infected district the people had got reinfected. He had seen a man at the Seamen's Hospital, London, who had never been out of the City, who had typical elephan-

tiasis, and the condition of his testicles, which Mr. Cantlie removed, also showed an allied condition. That man manifestly could not have had a filaria infection, and therefore the conclusion must be that there was some congenital stenosis of his main lymphatic trunks or that there was a tumour pressing on them. It therefore could not be said that all elephantiasis was a result of filarial diseases ; but, without the slightest doubt, the vast number of tropical cases were so. If one saw many cases of elephantiasis, and carefully examined the bloods of all of them at night, and also other bloods as a control, it would be found that the number of people showing embryos corresponded with the percentage of elephantiasis in the population. Dr. Sandwith's figure of 15 per cent. for Egypt was not too high ; it was a fair number. The example he mentioned of a doctor who took the trouble to stay up all night and examine a large number of cases in the hospital was highly interesting and inspiring, and he had proved how prevalent the trouble was, even though the clinical signs were not evident. Night examinations were very tedious, and that often made medical men disinclined to do the work. It was by no means pleasant to work in a native hospital at night ; the temperature was generally very high, and the smell was horrible ; in fact, the whole business was very discouraging in the extreme. Nevertheless, the work was interesting, and, by following it out, as in the case of other Tropical diseases, they might get knowledge of things which were now obscure. Medical men did not yet know all that was to be known about malaria, still less did they know all about beri-beri and other diseases like filariasis.

Dr. Sambon desired to point out that hyperparasitism in filariasis was a fact, and not a mere suggestion. Noya had absolutely shown in many cases the complete destruction of filariæ by nosema. It was a most important

question, and yet it had been scarcely considered. Howard, of America, had made a special study of hyperparasitism, and had shown that the prevalence of certain agricultural insect pests which affected plants was kept within bounds by hyperparasitism; that secondary parasites attacked the original parasite; that sometimes again there was a tertiary parasite which, by destroying the second parasite, favoured the first, and that sometimes there was even a fourth parasite. These were most interesting facts, and they must be considered as being highly important in the study of parasitic disease.

# FRITZ SCHAUDINN:

## A BIOGRAPHICAL SKETCH.

By W. CARNEGIE BROWN, M.D., M.R.C.P.

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To the ordinary student of Tropical Medicine, who reads Protozoology, not so much with the hope of acquiring special knowledge as with the intention of keeping *au courant* with advances made in a science that so nearly touches his own subject, there is no more striking fact than the repeated and authoritative citation of the name and work of Schaudinn. And when we reflect, further, that all this vast amount and variety of research was achieved in the short span of a life that did not extend to thirty-five years, that the facts adduced were, mostly, entirely new, that the work was addressed to men whose business it is to be wary and circumspect—men who have only too much reason to say of alleged discoveries that, if new, they are not true, and if true, they are not new—when we think of all these things, I say, we are constrained to ask the question, "What manner of man was this?" Yet, beyond actual workers in biological science, few in England know anything of the personality, of the human element in Schaudinn's career. Striking and phenomenal as it was, our own contemporary literature has almost passed it by, and it is only by treading somewhat unbeaten tracks in Continental records that one may hope to obtain even such a meagre gleaning of details as I venture to offer you to-night.

Frederick, or familiarly, and as he preferred to be called,

Fritz Schaudinn was born in the little village of Rosenengken, in East Prussia, on the 19th September, 1871. The cold grey Lithuanian skies now look down on peaceful and uneventful scenes, but, in bygone days, there had been tumult and stir enough on these troublous Russian Marches. Sixty-five years before, the stolid agriculturists had been startled by Napoleon and his legions as they thundered past on their way to Eylau and Friedland; dire poverty had not sufficed to save their homesteads from being utterly wasted during the military occupation of Konigsberg; they had lain in the track of the Madman of the North and his Goths in their fierce irruptions into Europe; and for seven long years their very existence had been at stake during the struggles of the Great Frederick for the safety of his people and the integrity of his dominions. Even as young Schaudinn was born the last echoes of the Franco-German war were but dying away, and the friends and relatives of the Schaudinn family were returning to their homes in East Prussia from the distant service with the national army; but peace and tranquillity had come at last, and the boy was able to obtain an excellent general education at the High School of Gumbinnen. At the age of 19, he passed his matriculation examination, and entered as a student at the University of Berlin. So far, his attention had in no way been directed to science. Rather had his bent been to the study of languages, and he went up with the intention of reading philosophy, or, as we would say, he entered for the Arts Course. But, in that curriculum, biology is an early and a prominent subject, and the introduction to it was a short course of lectures on the Protozoa. As by a flash of steel, Schaudinn was struck with fascination; he had found his *métier*; and to the study of the lowly organisms, which he then saw for the first time, he devoted his life.

At Berlin there were unusual opportunities. In Pro-

fessor Schulze, Schaudinn found a guide who appreciated and loved his brilliant pupil, and who knew well how to direct his work; and he had at his disposal the resources of a thoroughly up-to-date laboratory. Here he worked enthusiastically, as student and assistant director of studies, from 1890 to 1894; here he acquired his unparalleled skill of technique; and here he developed his extraordinary gift of interpretation. In the latter year he graduated, selecting as a subject for his thesis the life-history of the Foraminifera. He was able to describe many new species, but it was with the relations of the nucleus to the development and morphology of cellular protoplasm that his attention had been chiefly occupied. Although Dimorphism had long been known as a characteristic of Radiolarian life, it was Schaudinn's work at this early period that explained its dependence on nuclear influences, and called attention to their importance and significance. The views which he then formed as to the *rôle* of the nucleus proved, indeed, to be the keynote of his future work; and though not fully elaborated until several years later, they are the text of the sermon which he preached throughout his life. Briefly, these views were that in the nucleus of the protozoan cell there are two antagonistic elements—the one somatic, the other reproductive—and that the predominance of either is the characteristic and determinant of differing sex. In a male cell, the somatic or blepharoplastic element is developed at the expense of the female, which atrophies. In a female cell, the reproductive element develops at the expense of the somatic. In sexual fecundation there is a double fusion of antagonistic elements, with the formation of two bodies called by Schaudinn, Synkarions, which themselves again unite, and thus restore nuclear equilibrium. During schizogony, which occurs while the organism is parasitic, and therefore influenced by different extrinsic

conditions, nuclear equilibrium is upset, and one element, male or female, becomes predominant, to its own destruction. For, as the process goes on, it enfeebles the progeny, and unless sexual fecundation comes to the rescue as an alternating generation, the organism inevitably perishes. I do not pretend that this is else but the crudest summary of the results of innumerable observations and of a long train of exact mathematical and deductive reasoning; it is cited rather to explain the way in which Schaudinn's attention first became directed to the problems of tropical medicine, and as an example of his generalisations on reproduction, which, begun with the Foraminifera, were ultimately carried step by step through the hæmosporidia to the trypanosomes and spirochætæ.

The summer of 1894 Schaudinn spent at the marine biological station at Bergen in Norway, where he published a descriptive list of 139 species of Foraminifera, and on returning to Berlin, at the age of 23, he was appointed chief assistant at the Zoological Institute. Here he worked assiduously for the next four years, augmenting his slender salary by teaching as a *privat-docent* for the University, and issuing numerous reports containing much new information as to the life-history of Heliozoa, Radiolarians, and Amœbæ. What is regarded by zoologists as one of his best researches, viz., that in which he traced the sexual cycle of the free-living protozoan *Trichosphaerium sieboldi* through a complete series of alternations, was carried out at this time. During the summer of 1898 Schaudinn, to his great gratification, was sent with his friends, Römer and Friese, an artist, on a scientific expedition to Spitzbergen. Time is inadequate to follow him to the Arctic zone. It must suffice to say that the trip yielded the keenest enjoyment to the three comrades, and that the scientific results were sufficiently important to justify their publication in book form. Four volumes of this work,

*Fauna Arctica*, by Schaudinn and Römer, have already appeared.

On his return from the Far North, Schaudinn at once began what is to us the most interesting of all his work—the study of the pathogenic protozoa—by a research, in collaboration with Siedlicki, on the life-history of Coccidia. In the article on Protozoa in Allbutt and Rolleston's *System of Medicine*, Professor Minchin has given an admirably lucid account of this investigation, and of the development of *Coccidium schubergi*. The report of this investigation is of peculiar interest to us, not only on account of the extraordinary resemblance of the reproductive cycle of the coccidian to that of the malarial parasite, but also from the fact that it was traced by Schaudinn in the intestinal epithelium of a centipede about the same time that the evolution of the plasmodium was being followed by Ross in the mosquito. It may be noted, too, that it was at this time that Schaudinn first made use of the terms Schizogony and Sporogony. For his work on the Coccidia, Schaudinn was awarded the Tiedemann prize by the Natural History Society of Frankfurt, and was immediately thereafter called to the Imperial Office of Health in Berlin. Here he was appointed Director of the proposed Institute of Protozoology, for which he drew up a scheme of work, and, in consultation with the architect, suggested plans for the building. Pending the completion of the Institute, Schaudinn, who was now happily married, applied to be transferred to the charge of the Zoological Station at Rovigno on the Adriatic, where he hoped to be able to realise his ambition to repeat the observations that had been made on the tropical protozoan parasites. To his great joy his application was entertained, and he arrived at Rovigno in April, 1901. His first work there was a continuation of his Coccidian observations, this time as parasitic in a warm-blooded animal. He traced the development of *Cyclospora caryo-*

*lytica*, the pathogenic organism of a pernicious form of enteritis seen in the mole, through its various stages, and showed that the reproductive phase differed considerably from that of *Coccidium schubergi*. This research, even more than the others, involved protracted and continuous observation, and it is regarded by zoologists as being, even for Schaudinn, an exceptionally brilliant piece of work, but it is one that is somewhat beyond the scope of this paper. It may be noted, however, that Schaudinn believed that some of the appearances observed by him in this coccidian might explain certain phenomena in the development of tumours.

At the little village of San Michele, Schaudinn found abundant clinical material for his tropical work. One of his first studies there was undertaken to define the exact zoological position of the malarial parasites; and as a result, they were ultimately placed by him in the sub-order Hæmosporidia, and further divided into three species, Plasmodium vivax, Plasmodium malariæ, and Plasmodium immaculatum. The latter term being, however, found to be preoccupied, was afterwards changed to falciparum, and that classification is now accepted, and is the familiar arrangement which is followed by most authorities. Almost the whole of the malarial work published by Schaudinn referred to the first variety, that of benign tertian. He showed that relapses in this infection were due to parthenogenetic reproduction of macro-gametes which had persisted in the blood, and which, owing to their abundant store of cytoplasm, outlived all other forms. He stated that the familiar signet-ring form was characteristic of future schizonts, that it was due to the formation of a vacuole, that it was designed to increase body surface and, thereby, nutrition, and that no ring stage was observable in future sporonts. The latter, in consequence, grew slowly, and their sex had time to be differentiated, and was deter-

mined by the reaction of the host. He further described and drew from life the formation and fertilisation of the gametes, and again showed the extrusion of part of the nucleus and the actual process of fecundation by micro-gametes.

At the same time, he completed the researches he had formerly made on a hæmogregarine of the lizard, *Karyolysis lacertarum*, and demonstrated that the intermediate host was a tick, *Ixodes ricinus*, that the larvæ were infected by way of the ovary, and that the process was closely allied to those of the organism of spirillar fever in bed-bugs, and of the developmental spirochætæ of *Plasmodium ziemanni* in the mosquito.

More important still was his work at this time on Amœbiasis. At Rovigno, dysentery was endemic, and Schaudinn's familiarity with the reproduction of the free-living amœbæ stood him in good stead in unravelling the problems of the disease. He began at once a series of simultaneous observations on many different specimens, and one of the first results which he obtained, and which, perhaps, could have been secured in no other way than by his own method of work, was the one which solved so many hitherto unexplained difficulties. He showed, as you all know, that two species of amœbæ are parasitic in the human colon, which, though similar in appearance, differ widely in their life-cycles, in their reproduction, and in their pathogenicity. The research was productive of results which are now among the most familiar facts in tropical medicine, and it is unnecessary to say more than that, while carrying it out, Schaudinn did not hesitate to repeatedly infect himself by swallowing the developmental cysts both of *Entamœba coli* and of *Entamœba histolytica*, and that he had, in consequence, two serious attacks of dysentery.

In October, 1903, he published his famous work on the

life-cycles and development of *Plasmodium ziemanni* and *Hæmo-proteus noctuæ*, which are parasitic in the night-owl, and in which he followed the same organism through trypanosome and spirochæta stages. I understand that some of the statements in this report, which is unusually intricate and complex even for Schaudinn, are still the subject of debate by protozoologists, but there is no difference of opinion on the phenomenal power of penetration, and the profound interpretative ability which were displayed by him in his observations and arguments.

In April, 1904, while at Rovigno, and when he was deep in this tropical work, which appealed to him more than anything he had yet undertaken, and where he had now attracted a large following of eager disciples, Schaudinn was suddenly recalled to Berlin, to undertake what the authorities considered more practical duties. But, in the pursuit of knowledge, nothing seemed to come amiss. During the course of an inquiry which he was called on to make as to an outbreak of miners' anæmia in Westphalia, he seized the opportunity to verify Looss's account of the route of transmission of anchylostomum larvæ. Soon after this—in March, 1905—the medical world, which for a century had been searching the exudates and granulomata of syphilis for a micro-organism, and which in despair had almost abandoned the quest, was electrified by Schaudinn's announcement of the constant presence of a spirochæte in the lesions of that disease. Of this, too, which in future years will rank as, perhaps, the greatest of all his discoveries, it must here suffice to say that Schaudinn, though he fully realised the importance of his communication, was careful to refrain from definitely attributing to the organism an ætiological function, because he was unable to satisfy, at the time, the rigorous demands for logical proof which, in all his work, he imposed on himself.

With his mind still hankering after tropical work, Schaudinn, soon after his report on *Treponema pallidum* had appeared, asked for six months' leave to study at the Hamburg School of Tropical Medicine, and to complete his arrangements for joining Professor Koch in an investigation of trypanosomiasis in East Africa. About this time, too, he received from London what is stated to have been a most alluring offer. Possibly some of those here have fuller information of the circumstances of this temptation than is at my disposal; but, however that may be, the attempt failed. With pardonable gratification, the Continental record states that, after some hesitation, Schaudinn's patriotism triumphed, and even for the great stores of gold which Albion so lavishly bestows on her men of science, he refused to leave his people and his country.

In Hamburg, Schaudinn continued and extended his observations on the relations of spirochætæ and trypanosomes, and further studied transitional forms, such as he had noted in *Plasmodium ziemanni,* and which he believed to indicate metamorphic phases in the life of various protozoa, but his work was much interrupted by illness. Previously, as we have seen, he had had two attacks of amœbic dysentery; the first he had cured by the free use of calomel, but the second infection was of more malignant type, and it resisted treatment. He, perhaps, regarded it too lightly; at all events, he had no apprehension of the issue; and on the appointment of Director of the Department of Protozoology in the Tropical Institute at Hamburg being offered to him, he definitely accepted it. Before taking up duty which was so completely in accord with his tastes, it was, however, necessary for him to return to Berlin to start the work of the new Institute. There he spent the winter, with but slight improvement in his health, and when he returned to

Hamburg in March, 1906, he was still very unwell. From time to time he had severe dysenteric symptoms, with intermittent attacks of fever. On seeking advice he was ordered to take complete rest, and to try the effect of a change; but, having in the meantime been appointed representative of Germany to the International Congress at Lisbon, he persuaded himself that the anticipated benefit might be obtained from that trip. It was not to be. During the return voyage across the Bay of Biscay a peri-rectal abscess pointed and was opened, but with little benefit to his general condition. On arrival at Hamburg, seriously ill and suffering intense pain, Schaudinn was carried to a surgical nursing home. Evidence of deep suppuration round the sigmoid flexure was then discovered, and a further operation was undertaken to evacuate the pus which had formed. But it was too late; septicæmia had set in; the care and skill of his devoted friends and admirers were unavailing, and on June 22nd, 1906, he passed, aged 34 years and nine months.

So died Schaudinn, one of the greatest, if not the greatest, observer in natural science of all time. It is certain that his life was lost as a direct result of his disastrous experiment of self-infection, and it is more than probable that his health was undermined by his almost reckless industry. We are told that he would sit continuously for many days and nights, following at once the life-cycles of a series of different organisms, and that, like the chess-player who simultaneously engages five or six adversaries, he never forgot or confused the issues. He strove to observe life itself, and to work with material that was actually alive, and to that end, whenever it was possible, he discarded stains and dyes. And by this unwearying industry, by this marvellous facility of technique, by this phenomenal power of interpretation, by this almost

supernatural scientific *flair*, Schaudinn won for himself, in the short space of his life, a reputation that, in his own branch of science, is altogether without parallel.

In the preparation of these notes I beg to acknowledge the assistance which I have received from publications by various writers since Schaudinn's death, from the monographs of Professors Minchin and Calkins and Strong, in the later text-books, and more especially from a recent article by Dr. Langeron in Blanchard's *Archives de Parasitologie.*

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The PRESIDENT said the subject of Dr. Carnegie Brown's communication hardly admitted of discussion, but he was sure the Society was exceedingly indebted to him for the *résumé* he had given of the life of Schaudinn, and the account of the work that he had done. "Lives of great men all remind us"—they knew the rest. He wished that in this country some Schaudinn would arise in order that the corresponding Tropical Society in Germany could pronounce so eloquent an eulogium on him as Dr. Carnegie Brown had done upon Fritz Schaudinn.

The PRESIDENT having announced that the next meeting would be held on February 21, the proceedings terminated.

# KALA-AZAR IN THE ROYAL NAVY.

By Fleet-Surgeon P. W. BASSETT-SMITH, D.T.M. and H. Cantab.

(*February 21st*, 1908.)

While employed in a naval hospital at home, and dealing with diseases from foreign stations, occasionally it is one's fortune to meet with some of the more obscure cases of irregular fever and cachexia, the etiology of which is unknown, or only partially known. Of the latter, the disease called kala-azar, Dum-Dum fever, etc., is of peculiar interest, especially when attacking Europeans, or when it has been possible to demonstrate the parasitic organism. That the disease is more common than is usually supposed is very likely. I can look back on four cases that have been under treatment in my wards, in which the clinical evidences were very strong, though it is only in the present one that proof has been obtained, and for want of this proof the cases were returned as "splenic anæmia." In the latest account of the disease, by Leishman, the original endemic centre in Assam and Bengal has been widely extended, though definite cases from other regions are not common. He mentions Southern India, Ceylon, Burma, China, Egypt, Tunis, and Algeria.

In my four cases, the first contracted his illness in South Africa; the second was originally a merchant seaman, and had been much to India. In both the symptoms were typical, with very large spleens, very low white blood counts, and relative very low polymorphonuclear counts. In both, 2 cc. of blood were drawn off from the spleen, with negative results. These cases were fully reported in the

*British Medical Journal*, November 11th, 1905. I attribute the want of success in finding any parasitic organisms then to the large quantity of blood drawn off, and not to their being absent from the organ.

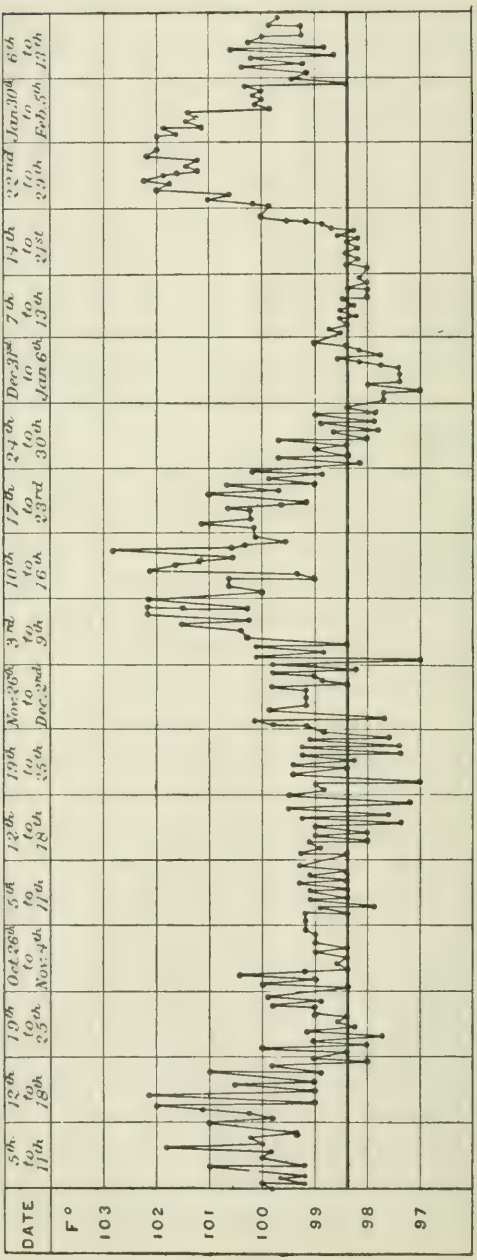
Case 3 was a chief stoker, aged thirty-eight. There was a history of chronic anæmia, commencing while in South Africa, attended with slight fever, recurrent hæmorrhages from nose, stomach, and bowel (at times severe), great enlargement of the spleen, a reduction of the red cells to 2,000,000, the white being 2,100, and hæmoglobin 40 per cent. The polymorphonuclears were relatively much decreased. In this case no parasitic bodies were found in the peripheral blood, and he refused to have any drawn off from either spleen or liver. Therefore, although all the symptoms pointed to kala-azar, it was not proved.

Last November a fourth case was received, being admitted for malarial cachexia. He gave the following history: From 1894 to 1896 he served on the East and West Coasts of Africa, and contracted malarial fever. From 1896 to 1901 he was on the Australian Coast. During 1902-03 he was on the East Indian station, and lived on shore in Bombay and Trincomalee. From 1904-06 he was in China, being quite well. He lived on shore at Hong Kong and Wei-hai-wei. He visited Shanghai, Tientsin, Hankow, Siam, etc. On arriving in England in 1906 he began to feel ill. He was treated for influenza for a month, then, improving, went to the Mediterranean. After three months, vomiting, anorexia, loss of flesh, pallor, headaches, and fever set in. He was treated for malaria, and invalided to England. On admittance to Haslar, he was seen to be a very old, cachectic-looking man; the skin was dry and earthy, he complained of headache, dyspepsia, epistaxis, and general weakness. His spleen extended to within 1 in. of the middle line and nearly down to the crest of the ilium, it was smooth and hard; the liver was also enlarged;

the urine was non-albuminous. The blood gave 3,800,000 red cells, and 2,000 white cells. A relative count gave polymorphonuclears 48 per cent., large lymphocytes 27 per cent., small lymphocytes 25 per cent.

There was irregular fever and a fast pulse. These symptoms continued, the fever being of an undulant character. During the height of the fever a small quantity of blood was drawn off from the liver, and Leishman bodies, both singly and in groups, were found. He was put on atoxyl, calcium lactate, iron, and bone-marrow; under this treatment, the atoxyl being pushed to 12 grains a day, the hæmorrhages stopped, the fever passed off, and he is now gaining weight.

A glance at the chart (p. 124) will explain how possible it is in such a case to confuse this condition with Mediterranean fever, as Bentley did, more particularly so, as I have myself had one case of eighteen months' standing, with such marked cachexia and splenic enlargement that I drew off some blood from his spleen to seek for the bodies, which blood instead gave a pure culture of the *Micrococcus melitensis*. Only 0.5 cc. was drawn off from the liver of the present case; it contained some liver cells, fairly abundant Leishman bodies, most of which are single and pyriform in shape, but collections of eight to fifty were found. The blood was sterile bacteriologically. A portion of the fluid drawn off was mixed with a citrate solution incubated at 20 deg. C., and daily examined for developmental forms. On the second day fission forms were seen, but from that day the bodies rapidly broke up, the swollen nuclei alone taking up the stain. The attempt at cultivating the organism was then only partially successful; the bodies on the second and third day were like some seen on slides of Indian cases which were given me by Leonard Rogers; they also resemble very much some stage in the evolution of trypanosomes as seen in rats.


OCT. 07.
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103
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5th to 11th
12th to 18th
19th to 25th
Oct. 26th to Nov. 1st
5th to 11th
12th to 18th
19th to 25th
Nov. 26th to Dec. 2nd
3rd to 9th
10th to 16th
17th to 23rd
24th to 30th
Dec. 31st to Jan. 6th
7th to 13th
14th to 21st
22nd to 29th
Jan. 30th to Feb. 5th
6th to 13th

I have ventured to bring these short notes forward, as further study and a better look-out for examples of these interesting diseases are required, for such cases may possibly be passed as malarial cachexia, etc.; also the marked improvement in his condition, at least for a time, under atoxyl treatment is noteworthy; lastly, all positive evidences of such diseases are worth recording.

The patient made so great a recovery that he has now returned to Australia, being quite free from fever and other serious symptoms for two months.

# A CASE OF KALA-AZAR: RECOVERY.

By Sir PATRICK MANSON, K.C.M.G.

(*February 21st*, 1908.)

The Rev. I. C., aged forty-four, a missionary, was admitted into the Seamen's Hospital Society's Branch Hospital, Royal Albert Dock, on November 1st, 1906, suffering from fever, night sweats, and enlargement of the liver and spleen.

The more important points of his previous medical history are as follows: He had had two spells of service in India, the first of seven and a half years (November, 1889, to March, 1897), the second, after eighteen months' furlough in England, of five and a half years (October, 1898, to April, 1904). While in India he was stationed at Calcutta and the Nuddea District, about eighty miles north of Calcutta. His duties took him about the surrounding country.

In May, 1890, he is said to have had enteric fever with a relapse. During his furlough in 1897-98 he suffered from neurasthenic symptoms—head tiredness, lapse of memory. Similar nervous symptoms supervened again towards the end of his second tour of service in India, induced, apparently, by severe domestic affliction.

The commencement of his present illness seems to date from about September, 1903, when he had the first of a long series of febrile attacks, which recurred, with considerable regularity, about once a fortnight. These continuing and his general condition gradually deteriorating, he was sent home, arriving in England in April, 1904. Dr. Harford, who saw him during the following July, informed me that at that time there was no enlargement

of liver or spleen. Subsequently Dr. Tirard saw the patient and reported that the febrile temperatures still kept recurring, although the night sweats from which he had suffered had ceased, and that he had gained weight.

Soon after this, temperature became steadily normal; weight went up, to 11 stone 2 lb., and in November, 1904, he was again able for clerical work in this country, although at times he suffered from feelings of mental fatigue. With the exception of during a short neurasthenic attack about the end of 1905 he kept at work till March, 1906.

The old febrile symptoms recurred, however, toward the end of March of that year (1906), and he was again obliged to give up work. On May 4th he was reported to be suffering from enteric fever. This diagnosis was evidently erroneous, the fever being of a very irregular character, sometimes having two or three rises in the twenty-four hours. The nocturnal sweatings again set in and were profuse. Later, on the supposition that the fever was malarial, large doses of quinine were administered, both by mouth and hypodermically, and kept up over a long period, but without permanent effect. On June 27th, 1906, the spleen was found to be enlarged and malaria parasites were reported as having been present in the blood—an observation, the correctness of which I doubt. Notwithstanding the continuance of the fever the tongue was clean, the appetite good. On the other, hand anæmia and wasting became pronounced.

When admitted to hospital on November 1st, 1906, he was seen to be markedly anæmic, sallow, wasted (9 stone 12 lb.). The tongue was clean, the appetite and digestion fair, bowels normal. A systematic examination of the various organs gave only negative results, except in the case of the liver and spleen, which were much enlarged, the former extending an inch below the costal margin in the mammary

line, the latter extending almost to the umbilicus. A blood count gave the following:—

| | |
|---|---|
| Red blood corpuscles ... ... ... | 3,100,000 |
| White blood corpuscles ... ... | 3,700 |
| Hæmoglobin ... ... ... ... | 70 per cent. |

Differential count of white blood corpuscles:—

| | |
|---|---|
| Polymorphonuclear ... ... | 37.5 per cent. |
| Large mononuclear ... ... | 24 „ |
| Lymphocytes ... ... ... | 29.5 „ |
| Transitionals ... ... ... | 9 „ |

On November 8th the liver was punctured with a fine needle, and in the liver tissue and blood so procured Leishman bodies were found in abundance. Diagnosis was now clear, the patient was suffering from kala-azar.

Treatment by intramuscular injection of atoxyl was at once begun, the dose being gradually raised from 10 minims of a $7\frac{1}{2}$ per cent. solution to 34 minims, about 3 grains. The injections were made usually every second day, sometimes every day.

On January 16th, 1907, in consequence of the appearance of symptoms suggesting arsenical poisoning, coryza, inflammation of the fauces and bleeding from the gums, the injections of atoxyl were suspended till February 15th, when the arsenical symptoms having subsided, the injections were resumed and continued till March 11th. On the latter date, the patient apparently making no progress, bone marrow tabloids were substituted for the atoxyl, but as the tabloids seemed to upset the digestion they, too, were stopped at the end of a fortnight.

The patient was now exceedingly weak, and had fallen in weight to 9 stone 1 lb., notwithstanding a fair appetite and good digestion. He had an irregular temperature, and, almost nightly, profuse sweats. He complained also of

pains and numbness in his legs, and also appeared to be in a hopeless condition. He left hospital on March 26th. I confess I expected to learn of his death within a few weeks.

From time to time I heard from him. At first the reports were not encouraging, but, as the summer advanced, they told of gradual improvement, of fever and sweats diminishing, of these being sometimes altogether absent, and, finally they spoke of decided improvement. From the beginning of July, 1907, there was no fever, although for a month or two longer the daily range of the thermometer was still abnormally large (96 deg. to 99 deg.).

After October the temperature became normal or subnormal. The nocturnal sweats had ceased in August. Weight steadily increased to 11 stone, the spleen and liver diminished in size, and he began to get about.

Wishing to satisfy myself that the good accounts I had received were well-founded, I visited the patient at his home in Norfolk on January 19th. I was gratified to find that there had been no exaggeration. The patient, who had left hospital the previous March, apparently in a dying condition, was indeed practically well. He could walk at a stretch about one mile, his weight was 11 stone 1 lb. (now 11 stone 3 lb.), his liver was of normal dimensions, and his spleen, which, when he left hospital, had extended well to the right of the umbilicus, though still enlarged was very much reduced, the anterior border being at least 3 in. to the left of the umbilicus. He complained a little of various paræsthesiæ, especially in his calves, sensations of wet, cold and stiffness; his knee jerks were somewhat exaggerated, and there was a suspicion of left ankle clonus. In other respects, though not vigorous, he was well, and he was craving to be at work again.

Several points of special interest have induced me to bring this case before the Society. There can be no

question about the correctness of the diagnosis: the clinical symptoms, together with the microscopical findings, are conclusive for kala-azar. The points of interest I desire to call attention to are as follows:—

1. After a chronic febrile illness lasting over one year (September, 1903, to October, 1904) complete cessation of all febrile symptoms, followed by restoration of weight, and some, though not full, return of physical vigour.

2. After a spell of comparatively good health for at least fifteen months (November, 1904, to March, 1906), recurrence of all the symptoms and in aggravated form.

3. After this severe relapse, during which the patient nearly died, apparent complete recovery.

4. Although at the time the drug did not appear to be of much service, the possibility that the atoxyl brought about or conduced to the recovery.

I may mention in conclusion, although I do not think it could have had anything to do with the fortunate issue of the case, that while in hospital the patient was inoculated with scrapings from a Delhi boil (Oriental sore), in which the characteristic parasite had been found some weeks previously. Two students were inoculated from the same boil at the same time as the patient, but in neither patient nor students did the inoculations "take." This failure may be attributable to the fact that at the time the inoculations were made the boil may have been in the involution stage, for shortly afterwards, I am informed, it began to show indications of healing.

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## Discussion.

Sir R. Havelock Charles, K.C.V.O., said he did not propose to criticise the very interesting paper to which they had listened further than to cite a case of successful treatment of the same disease. The symptoms the Presi-

dent had just narrated were almost a picture of those which his own case presented. The patient was a high-caste Hindoo, a native of the Behars, who had suffered from fever for about two years and six months. He had been under almost every form of treatment, including the Vedic system and the Western system, and there were very few drugs that had not been tried. He had been heroically treated by large doses of quinine, which had been given *per rectum*, by mouth, and hypodermically; he had had arsenic and iron and methylene blue; in short, he went through the whole materia medica; to say that was the easiest way to enumerate the drugs the unfortunate man had received. When he saw the patient, the spleen-dulness came down to the navel; the liver was a hand's breadth below the costal arch; the digestion was extremely bad; the tongue was at one time clean, and at other times foul; there was occasional vomiting, and profuse attacks of hæmorrhage, the latter of which very frequently brought him to the verge of the grave. Night sweats were very profuse, and weakness was intense, though at times his appetite was fairly good. It should be stated that the case had been diagnosed simply from the clinical symptoms, and not from an examination of the blood. The peripheral blood was examined, but it was impossible to make either a splenic puncture, a hepatic puncture, or a puncture of any kind; in fact, the services of the doctor who was in charge of the patient before he (Sir Havelock) was called in were dispensed with at a few hours' notice because he made such a suggestion. No malarial organisms were found in the peripheral blood, and an element of interest was one on which Dr. Bassett-Smith had touched, that it had been diagnosed as a case of Mediterranean fever. The patient was cured by fresh air at sea; he spent four months at sea, voyaging between Colombo and Australia, changing from one boat to another. He simply had the ordinary tonic

treatment. He confessed that when the patient went away he never expected to see him again. He experienced almost the same trouble in persuading the patient to go away which had overtaken his brother practitioner when he had attempted a splenic puncture, because none of his orthodox relations had ever been on the *Kala pani*, and they naturally did not wish him to go. However, the patient came back sufficiently well to pass a medical examination for Government service, and he was now cured, and in the Opium Department of the Behars. He (the speaker) would not have taken it upon himself to have said that this was a case of kala-azar without actual proof by finding of the bodies had he not seen so many hundred instances of that disease. In all respects the symptoms were typical of the disorder formerly known as malarial cachexia, or splenic anæmia, and now known as kala-azar; it could not be cured by quinine in any form, and the only point in which it lacked absolute certainty was the important one of detection of the bodies in the blood. Personally, he did not think that any drug would cure kala-azar; the best, and, indeed, the only treatment was complete change of air and immediate removal from the place where the disease had been contracted. With tonics and good food the constitution, even in this very grave infection, might be built up when things were apparently almost hopeless, and Nature, in this way, might ultimately cure the disease.

Major LEISHMAN said that seeing the title of the President's paper, which was "A Case of Recovery from Kala-Azar," he had come to the meeting hoping to get some clue to a successful treatment of this terrible disease There was no doubt that cases of recovery did occur—Rogers and others had reported them—but he did not himself attribute recovery in any of them to a particular

drug. Sir Havelock Charles thought that Nature was the cure, coupled with the benefits of change of air by means of a sea voyage. No doubt the latter might be beneficial, but he thought the cure, when it did occur, must be attributed to Nature alone. There was no evidence that change of air such as was given by a sea voyage was, *per se*, of any use. Soldiers who contracted the disease in India were invalided home, but the Military Hospital at Millbank was seldom without a case of kala-azar, and he had never heard of any favourable result following the long sea voyage which they made on their way there. He gathered from the remarks of the President and Dr. Bassett-Smith that atoxyl had done good, and he himself looked upon that treatment with very great hope. Atoxyl, like all arsenical preparations, was a dangerous drug, yet it seemed to give the most promising prospects they could point to, though his last case, he should state, was carefully treated by atoxyl, unfortunately without the slightest benefit. With regard to diagnosis, a suggestion had been made to him some time ago by one of his brother officers, Major Cummins, who was now attached to the Egyptian Army, which he desired to bring to the notice of the Fellows, because the method seemed to him to be worth a trial. It was well known that difficulties were experienced in detecting the parasites in peripheral blood, and, apart from the actual danger of splenic and hepatic punctures, patients objected to these procedures. There was not the slightest doubt, however, that parasites regularly circulated in the peripheral blood, and Major Cummins had suggested that artificial pustulation might be produced by some irritant applied to the skin, and that a collection of polynuclears could thus be obtained. In that way there must be a better chance of detecting parasites which, if any were present, would be englobed by the leucocytes. He ought to say, however, that he had within the last few days

examined pus obtained in that way from a case at Millbank without success. But the suggestion was a good one, and worthy of being thoroughly tested. There was another point in regard to development on which possibly some one of those present might be able to give further information, namely, the connection of the parasite as seen in disease with cultivation forms. The Society might remember that he had described the constant presence of some peculiar bodies, and that he traced their origin to irregular longitudinal fission which took place in the cultures during the flagellate stage. He knew, however, that many competent observers considered them to be merely artifacts. That was not his own impression. He mentioned this because a short time ago, through the kindness of M. Nicolle, the head of the Pasteur Institute at Tunis, he had received specimens of successful cultures made there from the spleens of kala-azar cases in Tunis, and in those preparations, although the development had not gone so far as he (the speaker) had been able to trace it, there were identical fission forms, little segments which seemed to split off one side of the flagellated parasites. He should be glad to have an opinion on that point, because his own impression that they were definitely connected with a stage of development was a very strong one.

Dr. Low said that he had listened with great interest to the descriptions of the cases, and he thought the only conclusion must be that, unless parasites were actually demonstrated by liver or spleen puncture, it could not be stated with certainty that any case was kala-azar. He had had opportunities during the past year of seeing many patients from the Tropics suffering from chronic enlargement of the spleen, often with enlargement of the liver—chronic cachectic cases—in which the Leishman-Donovan parasites had not been found. It was difficult to say what

the diagnosis should be, but there could be no doubt that there was a Tropical disease with these signs which was not associated with Leishman's parasite. Workers in the Philippines had reported similar instances. He remembered such a case in the Seamen's Hospital: An old gentleman had come home from India with all the signs of kala-azar, and though liver and spleen were punctured on three or four occasions, the parasite was never demonstrated. The patient eventually left the hospital and died, with his disease undiagnosed, somewhere in the country where an autopsy was not possible. Still another similar case was that of a man from the West Coast of Africa, whose spleen was punctured three times and his liver twice, the Leishman parasite never being revealed in either viscus. The patient also eventually left hospital, and he did not know what became of him. More recently again he saw, privately, with the President, a case which clinically was undoubtedly very like kala-azar. The blood count varied from 2,000,000 to 3,000,000 reds, while the whites were exceedingly low, about 2,000, and the polymorphonuclears showed a decrease. Sir Patrick Manson suggested the disease might be kala-azar, and he agreed with that opinion. The spleen was twice punctured, though he should say that in the first instance there was some doubt as to whether the needle had actually penetrated the spleen, but anyhow no parasites were found; and, although the liver was punctured on the third occasion, results were still negative. It was then thought the disease might be something else, but although the patient was carefully examined for other forms of fever no definite conclusion could be reached. Eventually he died, and fortunately he (Dr. Low) was able to obtain a post-mortem, at which it was found that there were double tumours of the adrenals, masses in each supra-renal capsule about the size of one's fist, the spleen was big, hard, and fibrous, and it weighed over 2 lb., but sections showed

no parasites. The tumours of the adrenals were found to be non-malignant adenomata, and he should mention that the patient never had any signs of Addison's disease, and there was no bronzing. That was a very interesting case, clinically indistinguishable from kala-azar, but revealed on the autopsy table to be of an entirely different character. He had since seen other cases, but unfortunately had not been able to follow them up so completely as the one he had just mentioned. Two of them were men from the Navy, one of whom had served on the East Coast of Africa, Zanzibar, and on various other stations, having had malaria once or twice. Personally he did not consider the disease in either instance resembled kala-azar, and he thought the spleens were more like those seen in splenic anæmia. He had, too, examined the blood in both instances, but never could find any parasites, and therefore the cases must be put down as doubtful. Ordinary splenic anæmia seen at home in people who had never been out of the country had a very close resemblance to kala-azar. They had similar blood counts, with big spleens, but the result of examinations made at Haslar had been that no Leishman-Donovan bodies had been found. There could be no doubt of the existence of such a disease, and that it closely simulated kala-azar, but what the nature of it was he could not say. Therefore, in a diagnosis of kala-azar, proof could only be admitted if the parasite was forthcoming, either as obtained from the spleen, from the liver, or from the peripheral blood. Though liable to be missed there, it had been frequently found in the peripheral blood by Donovan and Rogers. If the case was one of kala-azar, the parasite ought to be got after not more than two punctures of the liver. He thought it safer to puncture the liver rather than the spleen, because several deaths had resulted from the latter procedure, and a constitutional reaction followed if needles were inserted indiscriminately into that organ. To

sum up, he thought it should be laid down as a principle that kala-azar was due to invasion by Leishman parasites, but that other diseases, not only in the Tropics but at home, had very similar symptoms, and that a diagnosis could only be made by finding the parasite.

Dr. Harford remarked that he had taken peculiar interest in the case the President had brought forward, as the patient had been under his own care, he being responsible for him as one of the missionaries of the Church Missionary Society. The points of interest were that this began as a case of fever, about which there seemed nothing unusual, that it was met with in this country, that there was an indefinite history, and that the patient did not at first show any symptoms of severe illness. He had suffered from an attack of what was supposed to be enteric fever in the year 1890, and from that time there were evidences of nervous trouble, which had affected him ever since. He desired to ask the President to give his opinion as to what was the actual nature of the original fever the patient was suffering from when he came home in 1904. A little later in 1904, when treated with quinine by Dr. Tirard, it seemed to have a very definite effect, for the temperature became normal, and it was not until 1906 that serious symptoms showed themselves. Was this fever the real starting point of kala-azar, and were they to regard the disease as going back to 1904, and even earlier, in spite of the fact that serious symptoms only began in 1906?

Dr. Sambon said he could say little on the clinical side of the question, but he desired to refer in a few words to the remarks of Major Leishman as to the unequal division of the bodies. He had no experience of cultures of the kala-azar parasite, but in trypanosome cultures an exactly similar and unequal fission was witnessed, and he was therefore quite in agreement with what Major Leishman

said as to its being not an artifact, but an important stage in the development of the parasite. It was difficult to say exactly what that fission meant. He had thought that it might have something to do with sexual differentiation, and that in a sort of way the side-bodies might be compared to micro-gametes, but for the present that must be considered very doubtful. Another important point, to which the President called attention some time ago, was the similarity of the kala-azar parasite and the organism seen in Oriental sore. The parasites were so exactly alike morphologically that it was more than possible that there was some connection between them; and he believed that the suggestion of Sir Patrick Manson was a true statement of fact. Possibly the explanation was to be found in the intervention of a host belonging to the lower mammalia. In animals, parasites often attacked different anatomical structures to what they did in man, and produced altered effects, so much so that pathologists were now divided into two camps: one of which believed that a parasite in man was distinct from the same parasite as seen in an animal; the other, that it was in every way identical. That was the case, for instance, with tuberculosis, malaria, and many other diseases; the parasites were specifically distinct, and it was correct to give them different names. But it was quite possible that in course of time one species might pass into the other, and that the tubercle bacillus which came from the cow might be the progenitor of the one which had now become special to man. That was perhaps also the state of affairs in diphtheria and many other diseases; and he thought as the same parasite often affected different habits in man and in animals, that fact might throw some light on kala-azar and Oriental sore.

Dr. Sandwith said the suggestion of Colonel Leishman, which he was glad to know emanated from the Egyptian

Soudan, was extremely interesting; but he would like to ask the readers of the papers to say whether and what special precautions should be taken in puncturing the liver and spleen. He would be very sorry indeed to ask Fellows present how many times they had seen accidents occur from those procedures. Personally, he had seen several, and as kala-azar was a disease which, with the possible exception of Colonel Leishman's pustular experiment, had to be diagnosed, as Dr. Low said, entirely by puncture of the liver or spleen, that was important. Although there was a patient present who had successfully weathered the operation of having his spleen punctured—and there must be hundreds who had done so—still there was some danger connected with the procedure, and he put it to the authors to state definitely their opinions as to whether the danger was real, and if so, what steps could be taken to avoid or mitigate it.

Fleet-Surgeon Bassett-Smith said, in reply, that two main points had to be considered, first of all diagnosis, and secondly, prognosis. With regard to the first, it seemed that the most important point was the distinction between so-called tropical spleno-megaly and kala-azar. Personally, he held it was quite wrong to make a negative diagnosis even after splenic puncture, without result, when other symptoms were clinically typical. Without post-mortem evidence one could not negative absolutely the possibility of the Leishman-Donovan bodies being present. On the other hand, there were undoubtedly cases where patients had lived abroad, and had fever there, where the blood was low in reds and whites, as in the case of Mediterranean fever that he quoted, and where the spleen was so large and hard that one thought Leishman bodies must be present. Such spleens were seen in Mediterranean fever and they might exist in other diseases and be due to other

toxins, protozoal or bacterial; but, on the other hand, in the majority of instances of men with poor blood who had come from India, Africa and China, one should be very careful of making a diagnosis of kala-azar, unless a smear made from fresh spleen or liver showed the parasite. Even in sections it was sometimes difficult to detect the bodies, and fresh sections should always be used. With regard to the question of the developmental forms his cultures, unfortunately, had not been successful. On one or two days fission forms were seen, and one occasionally saw a flagellum, but they were uninstructive and it was impossible to dogmatize on them. In regard to cases similar to those of the sailors mentioned by Dr. Low, one should be careful about making a negative diagnosis. It had been suggested that the parasites might be found in the peripheral blood, but considering how few white cells there were in a cubic millimetre he thought that must be difficult, although in India, Rogers said, it was not uncommon to be able to demonstrate them. The method recommended from the Soudan might be useful; it was undoubtedly worth trying. In reply to Dr. Sandwith's question as to the precautions to be taken when a puncture of the spleen was made, the procedure was as follows:—First, the patient was warned to avoid movement: there ought to be perfect sterilization of the skin; the puncture should be made with a small needle, and the blood should be withdrawn quickly; a firm binder should then be put on, the patient being made to lie quite still in bed for some hours afterwards. But he did not puncture the spleen now; he had had the unfortunate experience of losing one case, and he decided never to do it again. Liver puncture was, he thought, safe.

The PRESIDENT said there were one or two points to which Dr. Bassett-Smith had not referred on which he desired to make a few remarks. The question of diagnosis

had always been an exceedingly important and difficult one, and when some time ago Donovan made the statement that parasites could be frequently found in peripheral blood he made diligent search for them. Donovan had stated very distinctly that in extreme cachexia, particularly when intestinal symptoms were present, the parasite would certainly be found in the polymorphonuclear blood-cells, and in fact Donovan had shown him preparations in which parasites were actually to be seen in the polymorphonuclear cells. But unfortunately, although he searched long and hard, he had hitherto failed to find them in undoubted cases of his own, so that the method could not be looked upon, in a negative sense, at all events, as a reliable means of diagnosis. The suggestion of getting a large collection of polymorphonuclears by inducing pustulation, and at the same time pleasing the patient, was a good one, and the next chance he got he intended to put it into practice. He did not think one was justified in making a splenic puncture in any case. His own practice now was to make a liver puncture, and it was done very carefully by a small needle. The puncture should not be too deep for fear of wounding the large vessels; and if deep respiration was prevented by means of a flannel binder and rest insisted upon, with perhaps a little pressure put on the binder afterwards, the risk from hæmorrhage was very small indeed; at all events, in his experience, no unfortunate consequences had resulted. But before attempting a puncture of the organ he thought it was exceedingly desirable to make a blood count, and not to proceed to splenic or hepatic puncture unless the blood count indicated a possibility or probability of kala-azar. If a large number of red and white blood-cells were obtained, and there was a good blood count, he did not think it was justifiable to make a puncture. Some years ago he had a very definite warning on that point, when a

lady came to him saying that she was saturated with malaria. She had been sent home from India with this diagnosis of malarial saturation, and she was said to have an enlarged spleen. She certainly looked as if she had Leishman's disease, but on making a careful examination he found the liver slightly enlarged, although he could not find any spleen at all. Palpating about the abdomen, he came on an enormous tumour in the right pelvis, and accordingly thought that some uterine or ovarian trouble existed, and that the case was one which did not belong to his own department of medicine. He therefore decided to send her to a gynæcologist, but before doing so he took some blood smears, which he put in his desk and, not having leisure to examine them, they were left there. A little later the gynæcologist came to him and said he did not understand the case; he thought it was not uterine, but it might be ovarian, and he wished to see him (Sir Patrick) in consultation before proceeding further. The lady, therefore, came to his house a second time with the gynæcologist, and, on careful examination being made, they diagnosed a pelvic tumour, and decided that the right thing to do was to remove it at once. The patient was sent to a nursing home, the day of operation was appointed, and everything was in order for a serious surgical operation. Fortunately he remembered the blood films, and on examining them was horrified to find that the lady was suffering from leucocythæmia, and that what they thought was a tumour in the right pelvis was probably a dislocated spleen. That was the case. Had they proceeded upon their mature diagnosis to open the patient's abdomen, he had not the slightest doubt she would have died from hæmorrhage. He was now very careful to make a complete examination of the blood before deciding to "break skin," whether it were an abdominal section or splenic or liver puncture, because it might well be that the case was

splenic leucocythæmia. He remembered another case of quite a different character, where a gentleman meeting with an accident in China fractured his lower jaw. A period of bad health set in, and he came home, touring about the Continent for a little while trying different cures. By-and-by he gravitated to his (the President's) consulting-room, where on examination an enormous spleen and liver with a certain amount of cachectic appearance were manifest. The Leishman body had just been "floated," and he jumped rather abruptly to the conclusion that this was a case of kala-azar. The patient was a Belgian, and he subsequently went to see him in Brussels, as he wished to be sure of his diagnosis. The blood count came out at 6,000,000 c.cm. There was quite a respectable number of leucocytes, and the fever was not of a very marked character. The man, instead of suffering from anæmia, had polycythemia; nevertheless, he thought he would like to know what was in his spleen and liver, but on puncturing both organs he found no Leishman bodies in them. The patient was suffering from some other disease. He ought not to have made that puncture, but curiosity made his fingers itch, and he could not keep his needle out of the patient's spleen and liver; but it was wrong, and in future he should not do such a thing again. Sir Havelock Charles attributed the recovery of his patient to the tonic influence of a sea voyage, and possibly he was right. Both he and Colonel Leishman somewhat decried the use of specific treatment; but it might be the case that atoxyl was the cause of his own patient's recovery. He did not assert it was, although specific drugs undoubtedly existed which could influence parasites similar to those of kala-azar. Mercury certainly had an influence on the spirochætes of syphilis; quinine had an influence on the plasmodia of malaria; ipecacuanha had an influence on dysentery, and why should not this drug have a specific influence on the

particular germ of kala-azar—perhaps none more likely than kala-azar, to be influenced, for atoxyl undoubtedly had a powerful effect on trypanosomiasis—a disease clinically very closely allied. His next case of kala-azar he proposed to place upon atoxyl treatment, and he would do so with a certain amount of hope. Dr. Harford asked him what he considered to be the starting-point of his patient's disease. He believed the patient acquired the infection in India, the regular fever that occurred in 1904 being the commencement of kala-azar. He believed there was a temporary recovery under some influence—he did not know what it was—which persisted for nearly fifteen months, and then for some reason, which he did not pretend to understand, there was a recurrence of the disease. He had had one case from which, almost with precision, the incubation period of kala-azar could be deduced. It was somewhere about ten or fifteen days. A gentleman went out to Calcutta, proceeding at once to a district very near to where the present patient came from, and ten days after his arrival he caught fever, from which he never recovered. That was proved to be a case of kala-azar, and it indicated a short incubation period, but it was reasonable to believe that the disease might remain latent for a long time. With regard to the bodies which Colonel Leishman mentioned as being split off like scales from the side of his culture forms, he had been struck with that feature in the drawings, and had wondered what they were. He could make no suggestion as to their real biological value, but they were facts to be noted, because they were undoubtedly not there for nothing; probably they were an essential element in the life-history of the parasite.

# PHENOMENAL ABUNDANCE OF PARASITES IN THE PERIPHERAL CIRCULATION OF A FATAL CASE OF PERNICIOUS MALARIA.

By J. CROPPER, M.A., M.D., Cantab.

(*February* 21*st*, 1908.)

The slides accompanying this paper were taken in Palestine from an old Samaritan who was admitted to the C. M. S. Nablus Hospital, Palestine, on September 15th, 1907, having, as he said, suffered from fever for twelve days, which he contracted in the Jordan valley, from which he had just come.

When first seen as an out-patient, pigmented, half-grown parasites were fairly numerous in the blood ; crescents were quite rare. The man was somewhat cachectic, but not at the time feverish, and being a Samaritan, his condition did not attract special attention. Though fatal cases of blackwater fever have occurred in Jerusalem and elsewhere in Palestine, this is the first definite case of death from an attack of pernicious malaria that I know of personally. On the evening of admission the temperature rose to 102 deg. or 103 deg., and sporulating forms were seen in the ear blood. Next morning the temperature was normal, and most of the parasites were of the unpigmented ring form. The patient had no bad symptoms, and wishing to see if the temperature in his case would follow a strictly tertian type, quinine was not given.

On the second morning after admission algide symptoms appeared; the temperature in the mouth was under 95 deg.,

and was with difficulty raised by hot-water bottles to 97 deg. Quinine was given in 15 grain doses by the mouth and hypodermically, but though he remained conscious, and could answer his name to within two hours of his death, he never rallied, and died at about midnight. Towards the end diarrhœa, which could hardly be described as dysenteric, set in. No post-mortem examination was allowed.

Most of the cases (100 in all) that came for treatment to the hospital during the five autumn months when I was there (though 50 per cent. were subtertian) were of so mild a type, that they would rarely consent to come into hospital, but preferred to return to their own villages; and this fact is enough to put one off one's guard in a country where fatal pernicious attacks are almost unknown. The slides described below were mostly taken two hours before death, and are stained by Giemsa's method.

## Slides Described.

1 and 2. Clumping of infected red cells, each containing a pigmented presegmenting body, suggesting a cause for the embolism or infarction of the different organs affected in pernicious attacks.

3. Very numerous unpigmented rings; of 500 corpuscles counted at random from 40 to 50 per cent. were infected, and in some fields more, so that in one field 100 parasites can be seen. Up to this time twenty parasites in a well-spread field was the most I had seen.

4 and 5. A red cell containing six unpigmented rings; in another slide (5) two corpuscles can be seen in one field, each containing five rings.

6. Subtertian gametes or crescents in all stages of development.

Towards the end the number of crescents increased in a

marked manner, four, five, or six were seen in a single field, and suggest to my mind some sort of migration from the bone marrow on the onset of algide symptoms. I give this for what it is worth. In very few cases are the crescents mature, and nearly always straight and fusiform, not crescentic; the pigment is nearly always discrete. The young male crescents are often perfectly circular in outline, and occupy the centre of the cell, being surrounded by a ring of hæmoglobin, the female gametes being sharply pointed. They can be distinguished from young segmenting forms by the fact that the pigment in the latter is nearly always gathered into a compact mass, even at an early stage. In any case, the chromatin has no definite position in the cell, and in rare cases exists as a distinct band across the equator.

7. Three sporulating bodies in one cell.

8. Three presegmenting forms in one cell. These, though somewhat rare, tend to show that when double or treble infection occurs each parasite goes on to full development, and effectually precludes the occurrence of any form of endocorpuscular conjugation, as described by Craig, of the American Army, in the Philippines.

9. Polymorphonuclear leucocyte containing sporulating body. (This only occurs in subtertian malaria.)

10. Leucocyte containing three sporulating bodies.

11. Pigmented leucocyte containing twenty-five pigment masses, each indicating one sporulating parasite; the protoplasm of the cell is very much enlarged.

### Addendum.

Besides the above the general condition of the blood deserves notice.

Nucleated red corpuscles are common and easily found.

There is evidently a considerable leucocytosis, though I regret that no count was made with fresh blood.

The differential leucocyte count is as follows—in a count of 500 :—

| | |
|---|---|
| Polymorphonuclear... ... ... | 40 per cent. |
| Large mononuclear... ... ... | 21 „ |
| Large lymphocytes ... ... ... | 26 „ |
| Small „ ... ... ... | 12 „ |
| Eosinophils ... ... ... ... | ·5 „ |
| Mast cells ... ... ... ... | ·2 „ |

## Discussion.

Dr. Low pointed out that such cases as Dr. Cropper had described were by no means common. Personally, he had seen sporulating malignant parasites in the peripheral circulation in about three cases out of 500 or more. He had seen leucocytes with two sporulating bodies inside, and he had also seen a single leucocyte with five developing parasites, and in one case, which was a very severe malignant infection of a rare type, 20 or 30 per cent. of the red corpuscles were affected. In less than 1 per cent. of malignant cases had sporulating parasites been seen in the peripheral blood.

Colonel Leishman said that, as far as his experience went, it was extremely rare to find sporulating bodies in the peripheral blood. He could call to recollection three cases in which he had seen them, in one of which the blood was taken just before death. The specimen under the microscope which showed the sporulating form in a leucocyte was to him extremely interesting, because he had not seen that particular phenomenon before. He had seen the individual spores frequently enough, but not a whole rosette actually mature. An interesting malarial film, not of pernicious but of quartan fever, had reached him a few days ago from Hong Kong. He had not many particulars, and

he could not absolutely say that the man died from malaria, but it was a fatal case, and he had never seen so many quartan parasites in the blood before. A smear preparation taken from the spleen was simply swarming with parasites, as was also the peripheral blood.

Dr. P. W. Bassett-Smith said that the malaria seen at Haslar was mostly non-malignant in type, and cases were generally mild infections. He had practically no experience of the malignant forms shown by Dr. Cropper, but it was interesting to know that such perfect examples of rosettes could be obtained in peripheral blood, because it was very rare indeed to be able to demonstrate them to a class, and, from a teacher's point of view, it was very desirable to describe and show complete life cycles.

A Fellow enquired whether the author thought the case shown had any bearing on blackwater fever. He had examined a number of films of blackwater peripheral blood in Northern Rhodesia, and on two occasions he had found what looked like a sporulating parasite. The red corpuscle was apparently just breaking up and, stained with methylene blue, the parasite looked like a bunch of grapes coming out of it as it broke up. Only on two occasions, however, had he seen that condition, and, as a rule, in blackwater fever no organism at all was found, but one generally found in the blood a large number of red corpuscles which had apparently suddenly broken up.

The President stated that under one of the microscopes there was a large clump of fourteen or fifteen parasites occupying a number of red blood cells congregated close together and clumped. He desired to know what interpretation Dr. Cropper put on that phenomenon. Probably a similar condition obtained in the vessels throughout the body, at all events in some particular organ of the body.

That might be an explanation of the thrombosis which sometimes resulted in pernicious forms of malaria, certainly the cluster shown was suggestive that malignant parasites might clump, and in that way possibly originate thrombotic clots. He further desired to ask whether the phenomenal abundance of all stages of malarial parasites in the peripheral circulation was the cause or the consequence of the malignant condition? Did their escape into the peripheral circulation cause the algid crisis, or was it the algid state that caused their escape? A remark had been made about the presence of rosettes and sporulating forms in the leucocytes. These were generally seen in the large mononuclears, but on one occasion in a preparation of Dr. Low's he saw a complete sporulating form in a polymorphonuclear leucocyte, and these were not generally credited with phagocytic properties in malarial infection. He was sure the Society was exceedingly indebted to Dr. Cropper for the finest microscopic demonstration of malarial infection he (the President) had ever seen.

Dr. Cropper, in reply, said he thought sporulation in the peripheral circulation must be commoner in some countries than in others. In his own experience of 480 cases he saw sporulation of the subtertian form in peripheral blood at least fifteen times, and in two of those cases he saw sporulating forms in the blood of the same patient twice within a year. With regard to the connection between such an infection and blackwater fever, personally he had not seen a single instance, but he knew genuine cases of blackwater had occurred in Jerusalem. One, a very definite case, was reported in the *British Medical Journal* by Dr. Masterman, but it was a fact that blackwater fever was extremely rare in Jerusalem, while malaria was abundant. With regard to whether the number of parasites was the cause or the consequence of algid symptoms, he

thought in such cases that parasites attacked the fever centre, upset its action, and at the same time caused general alteration in the blood pressure. As a rule, one did not see crescents that were not fully formed in the peripheral circulation.

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NOTE.—More probably algide symptoms in malaria are a direct result of infection of the intestinal mucous membrane by the parasite, producing the same train of symptoms as in cholera and other allied conditions, whatever the exact mechanism of these may be. Malarial hyperpyrexia, on the other hand, is no doubt caused by disturbance of the thermogenic centres of the brain by the same parasite.—J. C.

# ON THE RÔLE OF FILARIA IN THE PRODUCTION OF DISEASE.

W. T. PROUT, M.B., C.M.G., Liverpool.

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(*Friday, March 20th,* 1908.)

I FEEL that I owe an apology to this Society for again bringing before it the subject of filariasis, as one aspect of it was so recently and ably considered in the very interesting paper which was read by Dr. Low, and which went over some of the ground which I must necessarily cover; but my excuse must be that when I was asked to read a paper, I was unaware that a paper on the same subject was contemplated, and I had already been collecting material. If, then, I am unable to lay anything very new before you, I must crave your indulgence, and trust that the point of view from which I wish to approach it may be of sufficient interest to pardon any repetition, especially as I am inclined to take a different view of the relationship between filaria and disease from that which is generally held.

And, in the first place, it is necessary at once to exclude a number of filariæ from the scope of this paper. Of *Filaria demarquayi, F. ozzardi, F. perstans,* and others, little is known as regards their life history, and no pathological condition is yet regarded as being associated with them. It was suggested, tentatively, that *F. perstans* might be the causal agent in sleeping sickness; we now know that it is due to a trypanosome. The disease known on the West Coast of Africa as craw-craw has also been associated with a filaria, but in my experience this is erroneous. In cases where there are filariæ in the blood we

are pretty sure to obtain filaria if we puncture a papule, especially as it is difficult to do so without drawing a little blood, and if we look carefully we will find another parasite in a large number of cases, namely, the *Acarus scabiei*. Craw-craw is, in my opinion, a very slipshod diagnosis to make; it is not a specific disease at all, but a term loosely applied by the natives of the West Coast of Africa to almost any form of skin eruption, including itch, ringworm, eczema, etc., aggravated as these are by dirt and the excessive action of the skin, and it can in all cases be classified among one or other of the well-known forms of skin disease.

I do not propose to discuss the question of *F. medinensis*. The clinical phenomena, inflammation, induration of the limb, sometimes sloughing and abscess, are recognised as being due to the presence of this worm and disappear with its removal.

## F. Loa.

There are, then, only two left of any importance, pathologically, namely, *F. loa* and *F. nocturna*. The adult *F. loa* is a worm whose habitat appears to be the connective tissue, and which becomes visible when it approaches the surface in any situation where the tissues are thin and lax, as, for example, in the scrotum, the penis, and the conjunctivæ, from all of which specimens have been excised.

Evidence is accumulating that a micro-filaria showing a certain amount of diurnal periodicity is the embryonic form of this worm, and I should like to bring before you a new case which illustrates this point, and two other cases where the presence of this worm was associated with certain cutaneous phenomena. The three cases were seen by me recently, and occurred in Europeans who had been for longer or shorter periods in the Calabar district of Southern Nigeria. In each of them the adult worm had been seen

passing, meteor-like, across the conjunctiva, but had not been excised because it had not been possible to call a medical man before it had disappeared. We must conclude, then, that the parent worm is still at large in the bodies of these gentlemen. In two of the cases I have not been able to examine the blood more than once or twice, and on each occasion the examination was negative. In the third I was able to get a series of slides, with the following results:—

First day.—12 noon, four; 8 p.m., two; 12 midnight, none.

Second day.—4 a.m., none; 8 a.m., none; 12 noon, two (this was a poor film); 4 p.m., none; 8 p.m., none; 12 midnight, one.

Third day.—4 a.m., none; 8 a.m., none; 12 noon, three (this was taken by myself).

In each case a space equal to an ordinary square cover glass was examined. The degree of blood infection was therefore not great, but the periodicity was well marked, filaria being found on each day at noon, only once being found during the night. I have not measured the specimens, but the worm has a sheath, and very much resembles *F. nocturna*.

In each of these cases the presence of the loa has been associated with skin phenomena, and a brief description may be of interest.

Case 1.—The patient first noticed, in February, 1907, an intolerable itching in the neighbourhood of the big toe, which lasted for about twenty-four hours. The patient would scratch the place, then a watery bleb would form, and sometimes a general puffiness of the part, and after a day or two this would subside. There would be a lull for about three weeks, and then another attack. This lasted for about two months, then the bleb-forming stopped, and since that time he has only had attacks of swelling and

puffiness. I saw him in October, 1907. There was then itching and puffiness of dorsum of right foot, which disappeared in a couple of days. Since then there has been no recurrence.

Case 2 has had similar attacks of itching, followed by swelling, but these have occurred on many parts of the body, head, arms, hands, feet, and legs. The symptoms were first noticed three and a half years ago. When the adult first passed over the eye, it caused a tickling, followed by soreness. It was last seen in June, 1907. I saw him in November, 1907. There were then no signs of any kind, and there has been no recurrence. It is interesting to note that with the onset of the cold weather the worm does not appear to come near the surface, and the cutaneous phenomena cease.

Case 3, the one in which I have found embryos; there have been no typical swellings, though the patient said that he had a swelling of the left wrist in July, 1907, which persisted for some time. He suffers, however, from a peculiar cutaneous irritability. Parts of his body itch, he scratches and causes excoriations, with here and there small subcutaneous, reddish maculæ. Otherwise his general condition is good. In none of these was there any glandular enlargement.

In a paper in the *Journal of Tropical Medicine* (November 16, 1903, p. 347), Sir P. Manson describes cases of Calabar swelling occurring in the Congo, and suggests their relationship to *F. loa*; and Dr. Habershon, in the same journal, also cites cases where the association of localized swelling was noticed. In the original case of *F. loa* described by Dr. Argyll Robertson, swelling of the legs and arms was also observed.

I think, then, that we have now a sufficient number of recorded cases to justify us in associating the presence of *F. loa* with Calabar swellings, but I am inclined to

attribute the local phenomena to the mechanical irritation of the adult worm as it forces its way through the connective tissue near the surface. The œdema which would result would be intensified by the rubbing and scratching which takes place as a consequence of the itching, and the intensity of the phenomena would depend, to a large extent, upon the sensitiveness of the individual's skin. We know, for example, how very differently a mosquito bite will affect different individuals.

### F. NOCTURNA.

But it is when we come to *F. nocturna* that we find an association with a long list of diseases, which include lymphangitis, abscess, varicose glands, lymphatic varix, lymph scrotum, elephantiasis, etc.—in other words, with a series of diseases connected with inflammation or obstruction of some part or other of the lymphatic system.

It is unnecessary for me to enter into a history of this worm, which is well known. Its association with the diseases I have mentioned dates from the seventies and early eighties of last century. It was then that Sir P. Manson published his valuable and interesting work on the periodicity of the embryos and their transmission by the mosquito, and made suggestions as to the connection of the worm in the system and the occurrence of certain diseases. I have only recently had an opportunity of perusing this work, and I must confess to a feeling of astonishment that these very valuable and suggestive observations, forming, as they really do, the foundation of the present progressive position of tropical medicine, should have been for so long allowed to pass with so little recognition from the medical profession, and apparently without their vast importance being realized.

Of recent years there has been a marked advance in our knowledge of the distribution of filariasis, the methods

of its transmission from the mosquito to the human host have been worked out and the presence of other blood worms has been demonstrated. But the knowledge of the actual mechanism by which the *F. nocturna* is said to produce disease remains much the same as it was twenty-five years ago, and I propose to examine briefly the reasons for and against the opinion that *F. nocturna* is the causal agent in producing the diseases which I have enumerated, especially elephantiasis. And it is important to remember that in the 'seventies, when this theory took its rise, the extensive geographical distribution of *F. nocturna* was not well known, as well as the very great variations in the amount of the relative infection of different peoples, and it was not an unnatural conclusion that this worm, whose habitat was the lymphatics, which was found in tropical countries, in which also a disease of the lymphatics, namely, elephantiasis, was common, a disease clearly associated with some form of obstruction of the lymphatics, was the causal agent in producing this and other similar diseases. And it has been generally assumed that this is the correct view, and considerable ingenuity has been shown in explaining the difficulties which have from time to time cropped up. I propose, then, for the sake of argument, to assume that elephantiasis is not due to filaria, to criticise as impartially as possible the various suggestions put forward, to examine the possible mechanism of its production, and to see whether, if possible, there is not another explanation which would account for the production of this and other so-called filarial diseases.

Now the argument relied on in favour of the causal relationship between *F. nocturna* and filarial diseases are mainly two:—

1. That the geographical distribution of endemic elephantiasis and *F. nocturna* coincide, and

2. That *F. bancrofti* is a parasite whose habitat is the

lymphatic system; elephantiasis, etc., are diseases due to obstruction of the lymphatic system, therefore they are associated. Of course this is putting it very briefly and crudely.

1. The coincidence between the geographical distribution of endemic elephantiasis and *F. bancrofti* is claimed as being one of the most important proofs of the connection between the two, and it is maintained that where *F. nocturna* is common, there elephantiasis is common, and *vice versa*. Now, the coincidence of the geographical distribution of elephantiasis and *F. nocturna* simply means that both are found universally over the Tropics. I do not propose to go into this at length, as it was so fully considered recently, but I should like to say that it was the disagreement between the knowledge of the prevalence of elephantiasis in a locality, and what I unexpectedly found to be the degree of filarial infection, which led me to doubt whether the connection was quite so clear. I had accepted the theory, but when I began to examine the blood in Sierra Leone, where elephantiasis exists but is not obtrusively common, and I found that a large percentage of the population was infected with filarial embryos, I must confess that I began to ask myself what the explanation of this was. In a paper on filariasis in Sierra Leone I showed that the average infection of the Colony and Protectorate was 21.4 per cent., varying from 17.3 in Freetown to 38.7 in one of the outlying districts. I also showed that the natives of Freetown themselves only showed an infection of 6.5 per cent. So far as filarial diseases were concerned, I showed that chyluria is very rare, that lymph scrotum is very rare, but that enlarged lymphatic glands are very common. Out of 1210 cases admitted to the Colonial Hospital in 1900, derived from all parts of the Colony, there were no cases of elephantiasis; in 1901 two cases of elephantiasis of scrotum and two of leg. In the Pro-

tectorate, where the invasion is great (much greater than in Barbadoes), a number of cases are to be found on searching, but they are not obtrusive, and are clearly not so common as they are in Barbadoes. Of course, in a scattered population such as is found in the Protectorate, it is very difficult to get an idea as to the exact percentage, but a medical officer of great experience put it down as about 5 per 10,000 as an outside estimate.

It is evident that, with varying degrees of infection of the general population with *F. nocturna*, we ought to have a fairly fixed ratio of elephantiasis, but up to the present we have no proof of this. The only thing we have to go on are impressions that the disease is common. I have taken the trouble to go through the Colonial Office reports, with a view to see what ratio the admissions for elephantiasis bear to total admissions, but unfortunately the reports are absolutely useless for this purpose, as diseases are lumped together under each separate system—"Diseases of Lymphatic System," etc.

We can find many statements like this: Dr. Finucane, speaking of Fiji, says that "it is almost impossible to find an adult person free from elephantiasis in certain districts," "Every growing Fijian child shows signs of 'waganga,' that is, a form of lymphangitis," "Most Fijians are liable to four or five attacks annually," and so on. Now, no doubt these are valuable as records of clinical experience, but we should certainly expect to find some *definite* relationship between the varying distribution of filariasis and elephantiasis. That up to the present, I submit, has not been shown, and until it is, this particular argument cannot have the force which it would otherwise have.

Now, in considering the question of the relationship between animal parasites and disease, we should, I think, be able to apply similar rules to those which obtain in the case of diseases caused by vegetable micro-organisms:—

1. We should be able to prove the invariable association of the parasite with the disease produced.

2. We should be able to remove the parasite and grow it outside the human body.

3. We should be able to re-introduce it into the human body or that of some lower animal and there reproduce a similar disease.

4. We should be again able to demonstrate the presence of the parasite.

With reference to the first, I shall show presently the invariable association of *F. nocturna* has not being clearly proved; indeed, its absence in the case of elephantiasis is cited as one of the proofs of its association with the disease, and this difficulty requires explanation.

The second condition has undoubtedly been fulfilled by the proof that the parasite grows outside the human body in the mosquito, and that the human host is reinfected by the medium of the proboscis. But, of course, it is not possible to experiment on the human being in the case of a parasite which may produce a dangerous disease, nor to introduce it into one of the lower animals, as a human parasite does not necessarily flourish in any of them, so that the production of the disease, experimentally, and the recognition of the parasite afterwards—the third and fourth conditions—are still wanting. We find, then, that up to the present we fail in several of the conditions laid down, and we are forced to endeavour to come to our conclusion by means of deductions from definitely ascertained facts in the human body and the possible behaviour and effect of the parasite.

And at the outset, in the case of filariasis, we are met by several difficulties. In the first place, it is not maintained, so far as I have been able to ascertain, that the presence of the embryo of *F. nocturna* is the pathological factor in the production of disease; if that were so we should also have

to admit that the other embryonic forms might also produce disease. The embryo must, therefore, be regarded as harmless; the adult worm is the pathogenic agent.

Now, the first difficulty we have to get over is the fact, which is generally admitted, that in the great majority of instances the adult worm appears also to be harmless. Sir P. Manson states:

"There is nothing in the life-history of *F. sanguinis hominis* and its relation to the human host incompatible with the perfect health of the latter. The amount of injury done by the immature parasite in its travels towards its permanent abode is so trifling that no serious disease can possibly result from it. The mature animal itself lies extended in a vessel, and is perfectly adapted by its size and shape for the situation which it occupies; it creates no irritation, and the small amount of obstruction which it may give rise to is readily compensated for by a rich anastomosis. The embryos move along with the lymph, and, being no broader than the corpuscles, readily pass the glands and enter the general circulation. Hence they give rise to no trouble but circulate as easily as the blood corpuscles. In fact, the parasite seems in every respect well calculated to live in perfect harmony with its host and not at all likely to be the cause of serious injury or disease."

Primrose, in an interesting article, states "that the victims of filarial infections do not necessarily suffer inconvenience from the presence of the worm or its embryos. In fact, it would appear to be the exception for pathological lesions to manifest themselves in persons thus infected." My own experience, as the result of many hundred examinations, coincides with this, and it is unnecessary to labour this point—we may take it as undisputed.

Now if we have a disease, the alleged causal agent of which is in the great majority of instances harmless, if the presence of the *F. bancrofti* in the lymphatics and the embryos in the blood is not necessarily productive of disease—if, in addition to this, we find exactly similar diseases with the same clinical phenomena occurring in non-tropical countries where filariasis can be excluded with certainty, it is evident that, to carry conviction, the actual association of the parasite with the given disease must be of the clearest and most definite description, and must

be brought out in a manner to admit of no possible doubt Under such conditions mere inference is a somewhat uncertain prop to lean upon.

Now in order to get over the difficulty of the parasite under ordinary conditions, which, it is evident, is in the minds of most observers who have written on the subject, we must assume that some change takes place in the parasite or in the individual, something must be added by means of which the parasite acquires pathogenic properties, and various theories have been brought forward, the principal of which I may mention:—

1. That some filarial diseases, such as abscess and lymphangitis are due to *death* of the parent worm.

2. That elephantiasis and chyluria are due to *impaction* of the parent worm in some part of the lymphatic system —in other words, that it is merely a mechanical obstruction. It has been further suggested that the impaction of the worm produces hæmorrhage and consequent organisation of the clot, with narrowing of the lymph trunk.

3. This suggestion carries the great authority of Sir P. Manson, and has been put forward in a very convincing manner: that elephantiasis takes place as the result of obstruction of the lymphatics from the impaction in the afferent lymph vessels of a gland, of the immature ova from an aborting female. But in addition to this lymphangitis must occur. Let us briefly consider these theories *seriatim*:

1. That certain filarial diseases—abscess and lymphangitis—are due to the death of the parent worm. Now, of course, the presence of a foreign substance in the human body will not necessarily produce inflammatory action, it must become septic, and we must at once admit the possibility of an adult worm becoming affected with a streptococcus and thus produce an abscess and secondary lymphangitis, though the streptococcus might produce it without

the dead worm. But there is a point that does not appear to have been sufficiently considered, and that is, that in a population with a general degree of filarial infection, death in the adult worm must be constantly taking place, unless we are to assume that once the worm is in the body its life is limited only by the life of the individual. Given a definite infection of, say, 25 per cent. in a population, and a definite life-history of the worm, death must be constantly occurring at about the same rate, viz., 25 per cent., and we should expect a similar proportion of "filarial abscesses" to occur. But I am unable to find any evidence of this, Glandular and lymphatic abscesses are undoubtedly more common in natives of tropical countries (not, I believe, among Europeans), and for the same reason that ulcers are more common, there is more opportunity of streptococcal infection. Natives go about bare-footed; wounds and abrasions are more common—these are not attended to—their habits are not always cleanly, and we have the natural result of a septic infection. It is very rare to find a native without traces of scars on the legs.

The literature of filarial abscess is not plentiful, though there are frequent references in several papers, but I have found an interesting paper by Dr. Preston Maxwell, on which I should like to say a few words. Dr. Maxwell attributes his cases to death of the parent worm, and gives a table showing twenty-three cases of abscess, which he calls filarial, and the only evidence which I can find is the presence of embryos in the blood, which is no proof at all in a highly-infected community, as it would be necessary to show that abscess occurred more frequently among infected individuals than among the general community. In only one case out of the twenty-three was the remains of a worm found, though a careful search was made—surely a very low proportion. It is difficult to believe that disintegration and absorption can take place so rapidly

in an acute filarial abscess as to leave no trace in a few days.

In lymphangitis, again, it is difficult to understand the mechanism. Dr. Low (*Journal of Tropical Medicine*, August 15, 1902) says "the onset of an attack of filarial lymphangitis may be provoked, especially in old cases of filarial disease, by any trivial scratch or abrasion, etc." But that is surely a very frequent history of any attack of lymphangitis; it simply means that a source of streptococcal infection had been provided, and unless it is suggested that the presence of *F. nocturna* produces some change in the human body, say, by altering the opsonic index, so as to predispose to streptococcal infection, it is difficult to understand what the influence of the filaria is. This is a point which is worthy of investigation. I must confess, then, to a feeling that the proof of the connection of lymphangitis, abscess, and filariasis is not of a very convincing description.

2. The second suggestion is that elephantiasis and chyluria are due to impaction of the parent worm in some part of the lymphatic system—that there is a mechanical obstruction. So far as elephantiasis is concerned, I do not think that this is now seriously urged. It is certainly difficult to understand how a single live worm of the calibre of the *F. bancrofti* could produce such total obstruction as to lead to almost complete stasis of the lymph stream. I can understand a *dead* worm producing a blocking in the main lymphatic trunk in the following way: If we watch a piece of thread floating along a sluggish stream, it will remain extended until the anterior end meets some obstacle; the pressure of the water makes it fold on itself, and this process may be repeated until the thread is twisted up. And so with a dead filaria. Losing the power of anchoring itself, which it must have, it drifts along the lymph stream, in some main trunk, until it meets with some narrowing

or kink, when it may fold up again and again, with the ever-increasing pressure behind it. But unless this occurred in the thoracic duct, it could not interfere extensively with the general lymphatic circulation.

I show here an illustration of the lymphatics of the lower limb, not, I may say, with any wish to teach the anatomy of the lymphatic system, but simply to illustrate my points, and I am sure you will agree with me that it is very difficult to see how any one worm can produce a complete obstruction of any given lymphatic area.

Chyluria is, however, a disease which is distinctly claimed as being produced by obstruction of the thoracic duct by filaria, and I believe the case upon which this opinion is founded is the classical one of Sir Stephen Mackenzie's, which was published in 1882, upon which I should like to say a few words. I do not wish to take up your time by going into details of this case, but, briefly, it was a case where there was chyluria, with the presence of embryo nocturna in the blood and urine. Subsequently the embryos disappeared from the blood, and the chyluria improved. Then an abscess formed in the upper part of the chest, and, concurrently, there developed a lung affection, which was shown at the *post-mortem* to be pleurisy and empyema. From this the patient died. At the *post-mortem* there was marked inflammatory changes at the upper part of the thoracic duct, with occlusion. There was no trace of an adult worm having caused the obstruction. To my mind, on reading the case very carefully, there is no evidence that the obstruction was caused by *F. bancrofti*, the presence of the embryos merely showing the presence of an adult somewhere, and it is important to note that though the chyluria improved before death, a very marked obstruction was found.

I have been unable to find any case where chyluria has

been shown to be coincident with the presence of an adult worm in the thoracic duct.

We must remember that chyluria is not a common disease in tropical countries where filariasis is common. In West Africa, where it has been shown that a high degree of filarial infection exists, I have not seen a single case in eighteen years, and have only heard of one. In Fiji, where a high degree of filarial infection also exists, Finucane states that chyluria is rare, and in other tropical countries the same may be said.

Chyluria occurs in temperate countries, but here also is rare. Osler describes a case of well-marked chyluria in which he was able to make a thorough dissection of the thoracic lymph vessels, which were perfectly normal, and adds that the pathology of the condition is unknown. We are, then, in this position, so far as chyluria is concerned, that it may exist without obstruction of the thoracic duct, and with obstruction of the thoracic duct, but I do not think that until we have more evidence of the presence of an adult worm in the obstruction, we are justified in definitely stating that this is one of the diseases which is due to *F. bancrofti.*

Another suggestion is that the obstruction of the thoracic duct may be produced by hæmorrhage caused by the worm, as evidence has been found of an organised coagulum in the duct. Here, again, this is mere speculation, and, as a matter of fact, it is a very difficult matter to distinguish between the results of organised clot and the fibrosis of inflammatory action. In any case, we should also have to explain why the worm causes hæmorrhage in some cases and not in others.

In connection with obstruction of the thoracic duct I should like to show here a case for which I am indebted to the kindness of Dr. Bernstein, as it illustrates the effect of obstruction in a certain position. You will see that it has

the appearance of a typical case of elephantiasis of both legs, and in a tropical country would have been accepted as a case of filarial elephantiasis, and the fact that no filaria were found in the blood would have been cited as a proof that it was caused by *F. bancrofti*, and that the obstruction had shut off the embryos from the blood-stream. But this case occurred in a country where the possibility of filarial infection was definitely excluded, and the obstruction was found to be due to a fibrosis of the lower part of the thoracic duct. The following extracts from the *post-mortem* will be of interest:—

"The lower extremities and abdominal wall below the umbilicus were the seat of brawny œdema. . . . The receptaculum chyli and the origin of the thoracic duct were involved in an extensive pre-vertebral fibrosis, whilst from the first lumbar vertebra that duct was dilated to the size of a quill pen, until its entrance into the subclavian vein. The inferior vena cava and aorta did not appear to be constricted by the fibrosis, nor did the larger vessels in the groins. The fibrosis in the neighbourhood of the receptaculum chyli contained some lymph glands, with well-marked, dilated varicose spaces lined with endothelium, and evidently lymph channels, similar channels appearing in the fibrous and adipose tissue round the glands. The enlarged, soft inguinal glands showed similar dilatation of the channels, together with some thickening of the fibrous trabeculæ. . . .

"The morbid appearances may be summarised as follows: Chronic peritonitis, with especially prominent perihepatitis, perisplenitis, and pre-vertebral fibrosis surrounding the lymphatics about the level of the receptaculum chyli. Anatomically the condition is explainable on the view that the fibrosis had interfered with the lymphatic course at about the level of the receptaculum chyli. Below this a varicose condition of all the lymphatics and an œdematous and hyperplastic condition of the parts drained by the affected vessels, but not of the mesenteric lymphatics or lacteals."

This case is interesting as illustrating several points: first, the situation of the obstruction in a case of double elephantiasis; second, that in this situation the œdema and hyperplasia involved the abdominal wall; and, thirdly, that a condition indistinguishable from Tropical Elephantiasis can be produced without recurring attacks of lymphangitis.

3. We now come to the third suggestion, which is that elephantiasis is due to the adult female aborting, from

injury or otherwise, to the discharge of the immature ova into the lymph-stream, to their impaction in the vessels leading to the lymph glands, and to the complete obstruction, with its resulting lymph stasis, which ensues. In addition to this, it is maintained that lymph stasis alone does not produce elephantiasis—some injury in the area of lymphatic construction must take place; lymphangitis ensues, the inflammatory products cannot be completely absorbed owing to the stasis, and elephantiasis results. It is, therefore, recognised that an erysipelatoid inflammation frequently recurring is an essential feature in the production of elephantiasis arabum. This theory, then, takes up a somewhat different standpoint. In those we have already considered the adult worm is said to be situated in the proximal end of the lymphatics; in the present case it is situated in the peripheral part, and the obstruction is produced between the worm and the thoracic duct. It is, as you will realise, an absolutely different point of view.

Now, the possibility or even probability of the adult female filaria aborting occasionally, as other females do, especially if it is injured, must be admitted, and in that case the immature ova would be discharged into the lymph-stream, and they have been identified more than once by Sir P. Manson. That this does occur, and that the occasional blocking of some of the afferent vessels of a lymph gland may take place, must also be admitted. But when I come to examine the distribution of the lymphatics in the different situations in which elephantiasis occurs, I find a difficulty, from a mechanical point of view alone, of explaining how the obstruction which is necessary to produce a complete lymph stasis can occur. Looking again at the illustration of the superficial lymphatics of the leg, at what point in the limb can a single worm be situated so that the lymph-stream would carry the ova so as to obstruct the *whole* of the superficial inguinal glands? Or if the worm

were situated in the deep lymphatic trunks which accompany the lymphatics, they would only obstruct the deep inguinal glands, which are small and unimportant, as there is not a free anastomosis between the superficial and deep lymphatics, and we should still have the superficial lymph circulation going on freely.

Or, looking at an elephantiasis of the scrotum, in what situation does the worm lie so as to block up the superficial inguinal glands which command that lymphatic area? The worm must be in such a position that it will command both right and left sets of lymphatic vessels, for it is necessary to assume the blocking of both sets of inguinal glands. Elephantiasis of the scrotum is almost always symmetrical, and it is important to note that in elephantiasis of the scrotum alone, the blocking must be limited strictly to that area, as is shown by the fact that the skin uniting it to the abdomen is generally healthy. If that skin is involved, we know that the blocking must be above the glands, as described in the first case.

Or, again, take the case of localized elephantiasis of the arm which I have shown. Where can the worm be situated peripherally in the scheme of the lymphatics, so as to produce this localized infiltration and hyperplasia?

It is unnecessary for me to illustrate separately every situation in which elephantiasis occurs, but you will find that the same objection holds good.

I must confess, then, that to me this mechanical difficulty seems to form a great obstacle in the acceptance of this theory as an explanation of the production of elephantiasis.

Then there is another difficulty. In cases where we have multiple elephantiasis, for example the scrotum, one leg, and one arm, we must assume three adult females, all injured and all aborting. It seems to me that here it would be more reasonable either to assume an obstruction of some of the main lymphatics in the abdomen, or else, as

I shall suggest presently, some common cause of peripheral origin.

It will be convenient here to consider briefly the absence of filarial embryos in the blood, which is associated with this theory. In a number of cases of elephantiasis an examination of the blood shows the absence of filarial embryos, and it is explained that this is due to the blocking of the lymphatics, the embryos being unable to gain the main lymph stream and thus enter the blood; while the presence of embryos in some cases is got over by suggesting that they are derived from a reinfection. In the first place, this assumes an absolutely impervious obstruction, for, of course, as the embryos can pass through the smallest blood capillaries, they can do so through the smallest lymph capillaries. I think I have shown how very difficult it is to understand how such a complete obstruction as is required can take place.

Then we must remember that in a population which is infected to a greater or less degree by *F. nocturna*, any disease, whether it be pneumonia or Bright's disease, or eczema or elephantiasis, ought to show an amount of filarial infection equivalent to the general infection of the population. In other words, if there is an infection of 25 per cent. in the general population and 75 per cent. uninfected, we would expect to find at least 75 per cent. uninfected in any given disease. Now, I do not think that it has been conclusively shown that the proportion of uninfected cases in elephantiasis is greatly out of proportion to the uninfected in the general population; we would require to examine a very large number of cases in a district, which has not hitherto been done. The only figures which I have been able to find are those given by Dr. Low, and these vary with the general infection of the population, though at a somewhat lower level. He gives the following results:—

| — | General Infection. | Cases of Elephantiasis. | Embryo. | Percentage of Infected in Cases of Elephantiasis. |
|---|---|---|---|---|
| St. Kitts ... ... | 32.8 | 14 | 3 | 21.0 |
| British Guiana ... | 16.6 | 15 | 1 | 6.6 |
| Barbados ... ... | 12.66 | 10 | 0 | 0.0 |
| Trinidad ... ... | 10.75 | 83 | 0 | 0.0 |
| Dominica ... ... | 7.63 | 2 | 0 | 0.0 |
| St. Lucia ... ... | 7.58 | 5 | 0 | 0.0 |
| St. Vincent ... | 6.0 | 2 | 0 | 0.0 |

These so far are hardly conclusive one way or the other, and a larger number of observations is required.

Now the question comes to be, Is there any other explanation which will fit into the facts, and will account for the production of tropical elephantiasis, without the necessity of assuming an obstruction by means of filaria? I think that there is. In most of the suggestions, it will have been observed that the explanations generally include some form of inflammatory action, and chiefly lymphangitis, and it is in this direction that I venture to think the solution should be sought. Radcliffe Crocker, in discussing the pathology of elephantiasis, both sporadic and endemic, says: "The disease is consequent upon an occlusion of the lymphatic channels of the part affected, independent of the cause and nature of the obstruction, and whether it is at the trunk or periphery of the lymphatic circulation." I have described one case where obstruction in a main trunk, a pre-vertebral fibrosis, caused extensive elephantiasis, and no doubt similar cases may occasionally be found in the Tropics. But I am inclined to believe that the vast majority of cases of elephantiasis in the Tropics is of peripheral origin, and is due to a lymphangitis arising

peripherally, caused by the introduction of a specific micro-organism, and resulting in the gradual narrowing and obstruction of the peripheral lymph vessels. Radcliffe Crocker points out that in a temperate climate erysipelas, either as a severe or diffuse cellulitis or from repeated attacks, is one of the most common causes of lymphatic obstruction. Sabouraud examined and cultivated the serum in a case of elephantiasis, and invariably found the streptococci of erysipelas, but the cultures in the intervals remained sterile. Phlegmasia dolens is another disease which may occlude the lymphatics and lead to attacks of elephantiasis, and repeated attacks of eczema are responsible for a certain number, though these are seldom extreme instances of the affection. In this form the peripheral lymphatics must be the first to be affected.

And in the Tropics, where all forms of microbic life flourish so luxuriantly, where people go about bare-footed and bare-legged, where abrasions and injuries of exposed parts are of daily occurrence, where there is a frequent lack of cleanliness, surely there is ample opportunity for the entrance of pathogenic organisms and resulting attacks of lymphangitis, thickening of the lymph channels, and eventual obstruction. Would a peripheral lymphangitis of the scrotum not account sufficiently for a localized symmetrical elephantiasis with healthy skin uniting it to the body? And would not the presence of a similar lymphangitis account for the formation of the localized elephantiasis of the arm which I showed you? In many cases they seem to me to be so strictly limited to a given lymphatic area as to preclude the possibility of a proximal obstruction, but to be perfectly explicable on the theory of a peripheral infection by a specific micro-organism gradually spreading up the lymphatics.

I then venture with all humility to suggest that research should take this direction, and that during the attacks of

elephantoid fever an attempt should be made to examine the serum and to cultivate it, and I shall look forward to the time when some skilled observer demonstrates the presence of some specific organism.

I have thus tried, very imperfectly, I am aware, to review the present state of our knowledge as to the relationship between filariasis and disease, but, of course, it has been impossible for me to consider all the arguments for and against within the limits of this paper. And while I do not take up the extreme position of denying the possibility of *F. bancrofti* being at times the cause of lymphatic obstruction and its results, I venture to think that I have at least made out a case for the Scotch verdict of "Not proven," and a plea for further research in the direction I have indicated.

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## Discussion.

Dr. Low, in opening the discussion, said that one could agree with most of the statements in the first part of the paper, especially with the view that craw-craw was not due to filaria, and was not a specific disease. In Africa he had seen many skin affections which were called craw-craw, but they were nothing but scabies, or some other common skin disease. Coming to the question of *Filaria nocturna*, the author had adopted an attitude of scepticism as to the claim that filaria was the cause of elephantiasis and other diseases usually associated with its presence. Dr. Prout stated: "I propose, then, for the sake of argument, to assume that elephantiasis is not due to filaria, to criticise as impartially as possible the various suggestions put forward, to examine the possible mechanism of its production, and to see whether, if possible, there is not another explanation which would account for the production of this and other so-called filarial diseases." In the adverse

criticism of a theory, one expects at least an equally good explanation to be put forward; but that had not been done, and on examining the paper one found that Dr. Prout went no further than to say that these diseases might be due to something else; there was no definite suggestion substituted for the filarial hypothesis. Dr. Prout further stated: "The coincidence between the geographical distribution of endemic elephantiasis and *F. bancrofti* is claimed as being one of the most important proofs of the connection between the two, and it is maintained that where *F. nocturna* is common, there elephantiasis is common, and *vice versa*. Now, the coincidence of the geographical distribution of elephantiasis and *F. nocturna* simply means that both are found universally over the Tropics." He disagreed entirely with the latter part of that statement. The author, apparently, had worked only, or chiefly, in Sierra Leone, and had not examined similar conditions in other parts of the world. Personally, he knew of places in the Tropics where there were practically no filariæ. For instance, in the denizens of the forests of British Guiana no *F. nocturna* was found — at least, he had never found that infection, and Dr. Daniel's experience was the same. He had made many hundreds of blood examinations of Waganda—the natives of Uganda proper—but he never found *F. nocturna*. So that there were at least two places in the tropical world where *F. nocturna* was non-existent. And if natives of those two districts were collectively examined, it would be found that neither elephantiasis nor any of the other filarial diseases occurred there. He had seen elephantiasis in Entebbe, but the people who suffered from the disease were not indigenous, they were natives of the Soudan, in whom *F. nocturna* was common. One of the most important premisses on which the author's conclusions were based thus fell to the ground. The next point to which he desired to refer was the statement that

the *F. sanguinis hominis* was compatible with the perfect health of its host, one of the older observations of Sir Patrick Manson, whom the author had quoted. He (Dr. Low) was by no means satisfied that an infection of *F. nocturna* was compatible with health in the human subject. For example, filarial embryos might be found in the blood of a patient who had no symptoms and who seemed to be perfectly well, but the future history of the case was the important point. When Dr. Prout read his paper, Dr. Branch recalled the fact that when he (Dr. Low) was in St. Vincent, he had found *F. nocturna* blood films from two of the nurses at the Colonial Hospital: now he could state that, about five years afterwards, both those women developed elephantiasis. The cases were then entered in his book as "symptoms *nil*," now they both had elephantiasis. He believed that disease depended on the number of filariæ present in individual lymphatics. Then the author had quoted Primrose, who, so far as he remembered, had reported a Barbadian case of filariasis in Canada. Primrose's words were: "It would appear to be the exception for pathological lesions to manifest themselves in persons thus infected," but who his authority for this assumption was he did not say. In the speaker's own paper he had quoted St. Kitts, where 32 per cent. of the people were infected with filaria, and where there was an enormous amount of elephantiasis, chyluria, varicose glands, and filarial abscesses. He had clearly proved that where there was a high endemic index of infection many cases of filarial disease were seen. They had to be carefully looked for, but they were there. For example, in Barbados, the local doctors, knowing he was interested in the subject, often showed him private cases of elephantiasis in whites; in this way he had seen many manifestations of the disease, which the ordinary individual would have missed. An erroneous impression prevailed that white people did not suffer from

elephantiasis, but if one went behind the scenes it would be found that it was common in both races. Leaving the geographical question out of consideration, he desired to pass to one clear point the author had made, namely, that we do not know the exact mechanism and action of *F. bancrofti* in producing elephantiasis and other diseases. Unfortunately, post-mortems were difficult to get, especially at the stage when one would most like them—viz., after the first attack of filarial lymphangitis—for by careful inspection it might then be possible to ascertain where the parent filaria was lying and in what way it caused obstruction. But, speaking pathologically, it was exceedingly difficult to explain how the filaria came to block the lymphatics. The author, and those who denied the existence of the filaria as a cause of elephantiasis, adduced cases of the disease seen at home. He had seen several such instances himself. The first case of elephantiasis he had ever seen was in a woman who lived in Vienna, and who had suffered from chronic eczema. Another was that of a city watchmaker treated at the Tropical School, who had never been out of London; and the author quoted a case of double elephantiasis of the legs seen in London. He did not see that these cases invalidated the theory that the filaria was the chief cause of similar conditions in the Tropics. It was agreed that gonorrhœa caused 90 per cent. of all cases of stricture of the urethra; but, on the other hand, such a condition might be due to traumatism and other causes. In the same way stenosis of the trachea might be caused by aneurism or other lesions; and it stood to reason that it was the same with filarial infection. Any condition which blocked the thoracic duct, such as a tumour pressing on it, might easily produce the necessary stenosis. And regarding the distribution of elephantiasis, it was manifest that the filaria was at least one of the causes which produced the condition. For example, elephantiasis

was exceedingly rare at home; one might see two or three cases in a lifetime; but if one went to a place where *F. nocturna* was common, probably twenty or thirty cases would be seen in a day. The author had said that it was difficult to understand how a single live worm could produce such complete obstruction as to lead to stasis of the lymph stream. Why only one worm? As a matter of fact, in those cases which had been dissected masses of worms, not lying in the long axis of the lymphatic, but inextricably coiled up together, had been found. Given, however, stenosis of a lymphatic, it did not follow that elephantiasis, or even swelling of a limb, would follow. Inflammatory conditions must be superadded, and he agreed with the author when he stated that those inflammatory conditions might be produced by a streptococcus. It was known that a streptococcus played an important part in the production of the inflammation. The first step was lymphangitis attacks, like erysipelas, which, unlike that condition, were not infectious, but which in other respects were exactly like a streptococcus infection. The cases most difficult of explanation were those in which there was a localised blocking of one limb, say, a hand, or a wrist, or a foot, or an ankle, or, as the author stated, the scrotum, in which both sets of inguinal lymphatics must be blocked. It was impossible to get absolute proof of the mechanism without more post-mortem examinations; but he thought most medical men were agreed that filaria was a cause of these conditions. Embryos of filaria were always found in varicose glands and in tropical chyluria, and the association was evident. Admitting certain points which had been made, he did not think the author had brought forward anything that was conclusive or definite. Dr. Prout criticised all our present ideas, but presented no alternative explanation except that of the streptococcus, and everyone agreed that the streptococcus played a part in the production of elephantiasis.

Mr. J. CANTLIE said he had no idea that he was to be called on to speak, and his knowledge of the subject was limited. The remark that Dr. Low made about not finding cases of elephantiasis in the street was true in all parts of the world. He remembered, after he had been in Hong Kong for five years, writing an article in which he mentioned that he had never seen a case of ovarian tumour in a Chinese woman, or elephantiasis in a man. He had only just written the paper when a man with a very large scrotum applied for treatment, having travelled 1200 or 1500 miles through the country to come to the hospital. The scrotum was removed, and the man went back again, showing himself to all the people on the way, and telling them he had been cured of his tumour, which weighed, on removal, about 49 lb. After that he (Mr. Cantlie) began to think that the majority of the men on the road along which that patient had travelled suffered from elephantiasis of the scrotum, judging by the number of patients who came for treatment; at least, that appeared to be the case. The surgical aspect of filariasis did not perhaps bear upon the question very closely, but he thought one point had scientific importance, namely, that the situation of the parent worm, or the obstruction, was always proximal to the elephantiasis to which it had given rise. If there was an enlarged filarial gland in the groin the worms lay upon the proximal side. The question arose, Would the disease from which the patient was suffering be benefited by removal of the glands? Various cases were reported in which operations had been undertaken for the removal of filarial glands in the groin, notably some cases by Colonel Maitland, of Madras, and the result was that not only were the enlarged glands removed, but the filariæ disappeared from the patient's blood. What was to be done in the case of a huge lower extremity? If the leg were amputated, would the disease be cured? If the removal of the glands

in the groin cured the systemic disease in Colonel Maitland's cases, would the removal of the lower limb in elephantiasis do the same thing? Colonel Maitland's cases were well known and they deserved careful consideration, from a surgical point of view, as a method of cure. With regard to the length of time that a man might have filariæ in the blood and not develop elephantiasis, he knew of a case in London which the Chairman had also seen, in which the patient had had filarial worms in his blood for twenty-two years, and still had no sign of elephantiasis, yet in all probability he would eventually get the disease. The man was born in the Straits Settlements, and possibly had filariæ in his blood from childhood; but that case did not prove that elephantiasis had nothing to do with filaria. According to the theory of Sir Patrick Manson, if the parent worm were uninjured, elephantiasis would not result; it was only when the channels were blocked up that elephantiasis ensued. In this case no injury had occurred, and therefore the parent worm had not aborted and blocked the parts. Filarial infection might be present without the patient or physician knowing it. It might show itself as a tumour or an abscess in any organ of the body, or in a hydrocele, or in chyluria, or in enlarged glands. He remembered visiting a man who had swollen glands in the groin, a plague tongue, with a high temperature, and who was delirious; there was plague in the house. The man was sent to the plague hospital, where he passed urine which was examined under the miscroscope. It was then ascertained that the man had chyluria, and was suffering from filarial fever with enlarged glands.

Fleet-Surgeon Bassett-Smith described a case of scrotal disease in a marine. He believed it to be syphilitic, but in every way it closely resembled elephantiasis, and the diagnosis was doubtful, as the man had been for some time in China.

Dr. Carnegie Brown thought that, although the majority of the Fellows would disagree *in toto* with the author in most of his conclusions, yet they would all admit that the discussion had prominent advantages. In a science which moved so fast as tropical medicine it was wholesome to have a periodical stocktaking of our knowledge on any given subject, and when one advanced a theory which ran contrary to generally-accepted ideas the review was all the more likely to be of a vigorous and radical character. He thought, too, the author was right in depending largely upon his own experience; that, after all, was the most valuable quarry from which they could dig out their facts, and most of them would rely on their own experience rather than on that of others. But he certainly could not support him in so completely disregarding recorded experience as to ignore what had been already proved. The author had also, he thought, neglected to emphasise the difference between elephantoid disease and elephantiasis. Dr. Low had somewhat minimised that point too, but still it seemed to be a very important one, for it contained the kernel of the whole matter under discussion. In elephantoid disease—by which, of course, he meant lymph scrotum, lymph varix, chyluria and other similar disorders—it must be held as proved that all the lesions were as certainly due to filarial infection as that malaria was due to infection by the malarial parasite. The author demurred to this on the ground that none of Koch's postulates had been satisfied in the case of elephantiasis and chyluria, but if they asked for Koch's postulates to be confirmed, when metazoal pathogenicity was concerned, they could never hope to have any definite proof of the origin of this class of disease. Chyluria was of three kinds: one, which occurred both at home and in the tropics, was termed idiopathic, and it was so-called because physicians applied the name to a disease when they could not explain its origin; the second variety

of chyluria was seen only at home, and it occurred in connection with the very mysterious pathological condition, mostly diabetic, which was known as lipæmia; and the third class, which included 98 per cent. of all cases of chyluria, was wholly tropical, and was seen in association with filaria. There was nothing new about the filarial theory of chyluria, and it was fully established in the very first work on the disease which was published—that by Dr. Lewis, then at Calcutta. That great authority said that tropical chyluria was invariably due to filaria; his exact words were: "I have now examined more than thirty cases of chyluria, and in every instance, in the urine or in the blood, or in both of them, I have found filaria." That was a clear enough statement of his view, and it was made more than thirty years ago. He understood Dr. Prout to administer a rebuke to the profession generally for having paid insufficient attention to the President's researches. Probably it was merited, but it seemed to him that Dr. Prout was a little inconsequent, because the whole trend and object of his paper appeared to be to upset the President's conclusions. If the President's book proved anything, it proved the very close connection between lymph varix, lymphocele, chyluria, etc., and filarial infection. That there were some pathological difficulties was, as Dr. Low said, perfectly true. Thus, in chyluria, the post-mortem appearances, both in tropical chyluria and in chyluria as it was seen at home, were inconclusive. Both varieties might show absolutely no lymphatic lesion. There was a case reported in Lewis's work—a case of McConnell's of Calcutta—in which the blood was swarming with filaria embryo, and in which there was very marked chyluria. When the woman died, a most careful dissection was made, and nothing abnormal was found in the size or condition of any of the lymphatics in any part of the body. The thoracic duct was patent and healthy. Again, there was the doubt-

ful tropical case cited by Professor Osler, in which the same normal condition was demonstrated. But in at least as many cases, both tropical and non-tropical, the lymphatics were obstructed and dilated. Dr. Hertz, at a meeting of the Medico-Chirurgical Society in that house about a year ago, had demonstrated a case of chyluria of doubtful tropical origin, in which a careful dissection was made. The thoracic duct was obstructed, and every one of the lymphatic vessels of the lower part of the abdomen was widely dilated, so that one could see that a direct communication existed between the lacteals of the intestine and the lymph sinuses which opened into the bladder; but with regard to the connection between elephantiasis and filarial disease, that was quite a different matter. Although there was strong *prima facie* evidence that *Filaria bancrofti* was accountable for elephantiasis, nothing was proved; there was still a doubt, and that doubt differentiated elephantoid disease from elephantiasis. The essential fact that in elephantiasis no embryos were found in the blood had not been surmounted. How could it be said that their absence supported the filarial theory? Let them suppose the case had been that worms were always found; would not that have been seized on as an argument—as it would have been a good one—for the filarial origin of elephantiasis? Yet the absence of worms could not prove both things: it could not prove that the disease was filarial and non-filarial.

Another feature of the President's work which suggested a connection between filarial infection and elephantiasis was the fact that lymph varix, and some of the other conditions, admittedly elephantoid, shaded often almost imperceptibly into true elephantiasis. As Dr. Prout had pointed out, the contrary view was supported by the fact that elephantiasis was not uncommon in Europe. Chyluria was also common enough in England. In Dr. Roberts's book on

urinary diseases, several cases of chyluria were described, one of which, he remembered, was that of a woman who had never been out of Lancashire. At the Clinical Society some two or three years ago, Sir Dyce Duckworth showed an instance of elephantiasis in a girl aged 20, who had never been out of England, and he had never seen in the tropics a more perfect specimen of the disease. There had been a good deal of loose writing, too, upon the subject of the blocking of the thoracic duct by an adult *F. bancrofti*. It was an anatomical impossibility. Yet in several text-books it was stated to be a thing that often occurred. The thoracic duct was comparatively wide; in its largest part the lumen was about 8 mm., and in its narrowest part about 4 mm., while the diameter of an adult *F. bancrofti* was not more than $\frac{1}{5}$ mm. Even if such a worm could tie itself into the knot which sailors call the Turk's head, it could not effectually block the thoracic duct.

Sir Havelock Charles said that he had listened with pleasure not only to the paper but to the discussion. He thought that Dr. Prout referred to elephantiasis particularly, and confined himself to denying the association of elephantiasis with filaria. It never struck him for a moment that he was making the same denial with regard to elephantoid diseases. The majority of those present, with the exception of Dr. Carnegie Brown, were evidently of opinion that elephantoid diseases and elephantiasis were due to the same cause. Before entering into that question, he suggested the possibility that those conditions might have no direct relationship with each other, but might be associated in the same way as kala-azar and ankylostomiasis. He had spent a number of very happy years in the Medical College in Calcutta, where he had many cases of elephantiasis, operative and otherwise. He had also a class of students to whom he was indebted for a

great deal of his knowledge; in fact, his best teachers had been his students. At the time he was primed with the writings of their learned President, and he naturally taught that elephantoid diseases originated in filariasis, and that elephantiasis was also due to the same cause. But a class of students who wanted to know asked questions. They put it to him in this way: "You say that elephantiasis is due to filariasis; you have made us examine blood at all times of the night, and we have found in cases of elephantiasis no filaria; but here is a patient with compound fracture whose blood is teeming with worms, while there is a man with a 40 lb. scrotum, and he has not one. Not only do we find no filariæ in the body; when we remove the tumour and dissect it, the result is the same, and cases that have been most carefully post-mortemed have shown no occlusion whatsoever in either the abdominal or the distal lymphatics. Another point raised was that in the dissecting room cases were sometimes seen in which there were calcified filariæ at the attachment of the mesentery and around the thoracic duct, but in none of them was there elephantiasis. When a clinical teacher was heckled like that day after day by students, he naturally began to look for a more plausible explanation. He therefore said to the class: "Let us take the history, examine the condition, and then consider questions of pathology." They were all familiar with the history of such cases. The disease began with itching of the scrotum, irritation, and redness, which was made worse by scratching; then the patient got fever and was laid up for a few days from time to time. Often there were sores on the skin, and in 90 per cent. of all cases of elephantic scrotum the patient had dhobie's itch. After a few turns of fever, œdema ensued; it subsequently disappeared, but as attack followed attack the tissues hardened, and the tumour gradually increased in size. Another important fact was that the neck of the tumour

was healthy tissue, and that the operation would usually heal in a week, the patient having in a month or two a practically healthy scrotum. If that had been a proximal obstruction, one would naturally expect that the neck of the tumour would become diseased. It did not; hence he looked upon scrotal elephantiasis as a peripheral lymphangitis, and he considered that the cause of the lymphangitis was not in the body but in the scrotum. In his opinion the condition was not due to filaria, but it might very well be due to the fact that there was some secretion by the filariæ which set up endo-lymphangitis, and thus originated the thickening of the scrotum. Although he did not admit filaria as being the cause of elephantiasis, he had, however, been much biased by their learned President's writings, and his position with regard to the whole question could only be expressed by the words of the Persian poet and philosopher:—

> "Myself when young did eagerly frequent
> Doctor and saint, and heard great argument
> About it and about; but ever more
> Came out by the same door wherein I went."

He had no hesitation in saying that the operation of taking cuts out of the skin in elephantiasis of the leg was absolutely unsurgical. His own plan was to make a circular incision above the level of the healthy skin, and another along the side of the sole, because in such cases the sole of the foot was never affected. He then went round each of the toes and behind the heel, and took away the whole of the integuments right down to the deep fascia. The results were generally satisfactory; a patient would get quite well, and have no return of elephantiasis. If such cases had originated in obstruction, either in the groin or in the pelvis, or any other part, one would have expected the operation to have been futile, but it was not. His experience of chyluria in Calcutta was that it was always

associated with filaria. With regard to the removal of enlarged glands in the groin, he had performed that operation; and he remembered Sir Patrick Manson suggesting to him that in such cases he should implant the lymphatics in the veins. He found it an impossible surgical procedure, and thought it could not be done. One case was that of a Eurasian boy, who was a great cyclist, in which he removed the whole of the glands in the groin and femoral region. About eighteen months afterwards, the patient came back and said he had been told he had a hernia—an epiplocele. Knowing the history, he came to the conclusion that the tumour was dilated lymphatics round the cord. He removed from the cord a mass of lymphatics not unlike omentum, with the result that the patient completely recovered, and was able to continue his bicycling. If there was a worm lying *perdu* a little higher up it was still there, but the effect of taking the dilated lymphatic away was that the patient was cured. In conclusion, he thought there was no reliable evidence that elephantiasis was due to filarial infection.

The PRESIDENT read the following remarks:—Dr. Prout, in his paper, has given the arguments against the filarial origin of tropical elephantiasis and elephantoid disease, but he has stated only two of the arguments in favour of this hypothesis, namely, that founded on general geographical distribution, and that on the lymphatic location of the filaria. I confess, as he puts the case, these points are quite inadequate to establish the hypothesis. In discussing this subject we must be careful to discriminate between the tropical and the non-tropical forms of these diseases, for although their immediate cause is the same in all climates, namely, lymphatic obstruction, the agencies which lead to this obstruction are various, probably just as various as those which lead to ordinary dropsy. Elephan-

tiasis and elephantoid disease are specially prevalent in the Tropics. We claim, as the explanation of this circumstance, that they are specially prevalent there because in the Tropics, in the vast majority of instances, the lymphatic obstruction is brought about by that common tropical parasite, *F. bancrofti*; and we claim to have practically proved this in the case of the elephantoid diseases—by elephantoid diseases I mean varicose groin glands, lymph scrotum, chyluria and lymphatic varix generally—and all but proved it for true elephantiasis arabum. The proofs we rely on mainly are threefold—epidemiological, clinical, and post-mortem. As regards the first, the epidemiological, or, rather, the endemical, it includes, as Dr. Prout concedes, the general geographical distribution of disease and parasite, but we go further than this in applying the epidemiological test, we claim more than general geographical distribution. We claim that the *particular* local distribution, the *proportionate* geographical distribution, and, further, as Daniels and Low have shown in the case of British Guiana, the *racial* distributions of disease and parasite correspond. Dr. Prout concedes the lymphatic habitat of the parasite. That is something. But he does not mention that the adult filaria has been frequently found, and that its young are nearly always found in the particular lymphatic fields affected, as well as in the blood, in certain types of elephantoid disease. Thus in chyluria the embryo filaria can be found in the urine in five cases out of six, the same is true of the discharges in lymph scrotum and of the contents of varicose groin glands. I can vouch for this from a large personal experience, and I have no doubt Drs. Daniels and Low, besides many other observers, can do the same. Moreover, in these cases the parent worms themselves have frequently been found in the affected tissues. The relationship to the parasite is most intimate. Does Dr. Prout regard this as accidental,

or would he explain it by asserting that the diseased tissues attract the worms as a dunghill does sparrows? If so, how does he explain the well-established fact that in a community in which the filaria occurs in 9 per cent. only of the general population, it is present in 80 or 90 per cent. of the subjects of chyluria, lymph scrotum or varicose groin glands? Dr. Prout may retort that it is absent in a proportion of these cases, and that if it be the cause of these diseases, why is it not to be found in all cases? Our reply is, that in all cases it has been present at one time, but that, after producing the lesion in the lymphatics which eventuate in the disease, from some cause or other it has died. Now, as a matter of fact, on more than one occasion I have been, so to speak, present at the death of the filaria, and have watched the disappearance of its young from the circulation. Some years ago I was consulted by a young man for enormous varicose glands, which occupied his groins like huge femoral herniæ. They were quite characteristic, and an examination of his blood at night revealed a very heavy filarial infection, something from three to four hundred microfilariæ being found in every drop of finger blood. The patient was a plucky fellow, and always willing to lend himself to observation and experiment. I wished at that time to ascertain how long the individual microfilaria lives in the blood after its birth: does it die off in a day or two, or does it live for several days or weeks, or even longer? With the consent of the patient, I prepared an experiment which I thought might settle the point. Sir Victor Horsley very kindly consented to inject a large quantity of this patient's blood into a couple of monkeys whose blood I had proposed to examine subsequently from day to day, and so ascertain how long the injected filariæ lived. A day or two before the projected experiment was to come off, the patient was suddenly seized with violent rigors, high fever, and inflam-

mation of his groin and lymphatics. The attack came on while he was in the city attending to his business, and so suddenly and so violently that it was impossible for him to get home. He had to be carried to the nearest hospital —the London. It was more than a month before I saw him again. I then took him to Sir Victor's house, late one evening, when I thought the microfilariæ would be plentiful in his peripheral blood. Everything was got ready for transfusion, when it occurred to me that I had better ascertain by microscopic examination if the parasites were present in their usual profusion. Judge of my astonishment and disappointment when, instead of finding as formerly three or four hundred in each drop of finger blood, I found only three—a number, of course, quite inadequate for my purpose. The experiment had to be abandoned, at all events for the time. I continued to examine the patient's blood every fortnight, and at different hours of the day and night, during a period of months. I generally could find three or four per drop, but never more. Manifestly most of the parent filariæ had died. The patient kept at business in the City, coming to see me occasionally in the evenings. Then his visits suddenly ceased, and, when in reply to a letter, he came to see me again, he explained that he had had another severe seizure of lymphangitis, just as severe as the previous one, and that he had again been taken to the London Hospital, where he had lain ill for many weeks. Once more I examined his blood, and now not a single parasite could be found, nor, although I repeated my examinations subsequently many times, did I ever encounter a microfilaria. Manifestly the first attack of lymphangitis had caused, or had resulted from, the death of the great majority of the adult filariæ infesting his lymphatics, and the few that had survived the first attack had perished in the second. Nevertheless, the enlarged glands are still extant, although

the parasites which had caused them have disappeared long ago. Those who oppose the filarial origin of endemic chyluria, lymph scrotum, and varicose groin glands have to explain how it is that in the vast majority of these cases microfilariæ can be readily demonstrated in the blood and characteristic fluids, and in many cases the adult worm in the lymphatic vessels, and those who oppose the doctrine of the uniformity of the cause of these three conditions—as Dr. Prout asserts that Osler does—have to explain the frequency with which they occur and follow each other, as has now been shown in quite a number of published instances. I am sometimes tempted to think that those who criticise and oppose this view of the pathology of chyluria have not studied the cases I refer to. It is true the exact *modus operandi* of the parasite has yet to be explained, but that it does give rise to lymphatic varix, to my mind does not admit of the slightest doubt. But as regards tropical elephantiasis, I admit the case for the filaria has not been so completely and convincingly made out. As regards the epidemiological evidence, it is just as complete as for the filarial origin of these other conditions, but, owing to the general absence of the microfilaria in the blood in elephantiasis, the clinical evidence, at first sight, seems to be extremely defective. Nevertheless, if we consider the matter properly, this deficiency can be accounted for; indeed, it seems to be inevitable almost, one might say essential, for the establishment of the hypothesis. Apart from this, a certain amount of clinical evidence is forthcoming. Thus cases have been published in which genuine elephantiasis of the scrotum had supervened on lymph scrotum, and in which elephantiasis of the leg had supervened on the removal of lymph scrotum, and, also, in which elephantiasis had concurred with lymph scrotum in the same individual, and, also, in which one member of a family had lymph scrotum and another elephantiasis of the leg.

Surely these concurrences are not accidental. Is it not infinitely more probable that they are the effects of the same cause? And if it be conceded that in the cases of the diseases characterized by lymphatic varix the filaria is the cause, then the filaria must be the cause of tropical elephantiasis. Besides this argument, another just as cogent may be urged. I confess, however, that the data on which it is founded are not so extensive as I could wish. In a series of examinations of blood from natives of Cochin I found that the percentage of filaria-infected individuals was markedly greater in those who had no elephantiasis than in those who had. Dr. Low has made a similar observation in the West Indies. This, at first sight, would seem to militate against the view that the filaria was the cause of elephantiasis. But if we think of it, it is, on the contrary, a strong argument in favour of this hypothesis. I have already shown that the filaria sometimes dies out and that a cause of its death is lymphangitis. Now, in almost every case of elephantiasis, lymphangitis is an early and frequently recurring feature, and therefore the filariæ, at all events those in the affected lymphatic area, have a poor chance of survival. This, I believe, is one reason for the absence of microfilariæ in the blood in tropical elephantiasis. There may be other reasons, as, for example, that which I have conjectured—the plugging of the lymph channels by the ova of aborting worms. This I have seen on more than one occasion, and, indeed, it may well be the first step in starting in some instances the lymph stasis which, on the supervention of a bacterium, induced lymphangitis, ending in true elephantiasis. Dr. Prout apparently does not admit this argument. He points to the elaborate and extensive anastomosis of the lymphatic system. Why, I would ask, is this anastomosis so elaborate and extensive? It is because it is necessary. If part of it be obstructed, the affected area is, by so much, imperfectly drained. If

we consider the hundreds of thousands of ova in the uterus of a filaria, it is evident that there are emboli sufficient in one worm to plug a great many if not every afferent lymphatic capillary in any limb or two limbs. Dr. Prout speaks of applying Koch's tests for the validity of the claims of the filaria. Such tests are for many reasons inapplicable. We are dealing with a metazoal parasite and not with a bacterium; with a parasite which produces disease only occasionally, only indirectly, and only, so to speak, by accident: Koch's tests are not for such. Elephantiasis and the elephantoid diseases are the sequelæ rather of a long antecedent germ infection, and not the necessary effects of such an infection. They stand in the same relation to the original cause that dropsy does to the germ of rheumatic fever, or of stricture of the urethra to the gonococcus; and to these no one would consider Koch's postulates as applicable. I should have liked to have heard more in detail Dr. Prout's personally ascertained data on the strength of which he appears to reject the filarial etiology of elephantiasis and the elephantoid diseases. Formerly the filarial doctrine of elephantiasis was, I consider, too readily accepted; now, it seems to me, it is too readily put aside. Until one has fully studied at first hand all the published evidence for the theory—and there is a great deal more of this than is generally supposed—he should pause before rejecting it. I confess, as regards tropical elephantiasis arabum, the evidence for the theory is not complete or absolutely convincing, but it is such as to make the filarial doctrine of these diseases far and away the most plausible. For the elephantoid diseases, as I have said, it is conclusive. For many years I have been waiting for some tropical pathologist to take the subject up again. It is a very important one both theoretically and practically, and I can assure those who would seriously work at it at first hand a most interesting and fascinating study. Quite

recently, in the *Journal of Tropical Medicine and Hygiene* for April 15, Dr. Wellman, of Angola, expresses his disbelief in the filarial hypothesis of elephantiasis, founding his scepticism on his personal experience to the effect that elephantiasis is not uncommon in his district, but he failed to find filariæ in 500 slides of night blood from as many individuals of the same locality. If this correctly represents the facts, then the filarial doctrine of elephantiasis falls to the ground, but before this can be allowed, further statement of the facts of the method of investigation Dr. Wellman employed, of the localities which had been visited by his cases of elephantiasis, and so forth, must be submitted.

# NOTES ON AN UNUSUAL CAUSE OF DYSENTERIC DIARRHŒA IN THE TROPICS.

By Dr. W. T. PROUT.

(*Wednesday, April* 15, 1908.)

The small epidemic to which the title of this note refers occurred a number of years ago, when I was Chief Medical Officer of the Gambia. There was at that time a company of the West India Regiment stationed there, and as there was no Army medical officer available, I was for a considerable period the medical officer in charge, and it was among the men of this company that the outbreak occurred. It took place in the early part of the rainy season—that is to say, the weather was still fine and clear, but there were occasional heavy showers of rain. The men lived in large barracks, which formed two sides of a large square, and though old, they were clean, and on the whole in good sanitary condition. It is important to note that the water supply of the troops was derived solely from rain-water, which was stored in large iron tanks. Towards the end of the dry season, as the tanks became empty, they had been thoroughly cleaned and whitewashed inside, while the inlet pipes had been disconnected until the first shower had washed the collecting area, which was the barrack roofs. At the time of the outbreak the tanks were pretty well filled.

One day a man presented himself to me at the hospital, complaining of griping pains and diarrhœa, with a certain amount of blood in the motions. There was very little constitutional disturbance, very little tenesmus, and none of the serious signs of an attack of acute dysentery. I

naturally paid little attention to it, until one after another of the soldiers came to me suffering from the same symptoms. Now, at the beginning of the rains, when the water supply is very liable to contamination, diarrhœa is by no means uncommon, but at this time, curiously, there were no cases of this dysenteric diarrhœa among the civil population, so that it was clear that the cause was one confined to the troops. I had thought at first that some of the men had been drinking water in the town, but I was able practically to exclude this. The drinking water of the native population is derived from shallow surface wells, from 4 ft. to 8 ft. deep, and as Bathurst is simply a sand bank, the water is often brackish, and the men did not like it, after being accustomed to the pure rain-water. I need hardly say that the water in these wells is very much polluted, as you often find a cesspit a few feet away.

I had, then, to seek the cause in the barracks, and I must confess that after the precautions which had been taken with reference to the tanks, I did not at first examine the water very carefully. I had a bucketful run off, and it appeared clear, sparkling, and had no bad taste or odour. An enquiry into the food supply and a careful inspection showed nothing wrong ; the men absolutely denied having eaten or drunk anything outside which could have disagreed with them, and the barrack-rooms and latrine arrangements were in a perfect sanitary condition, so by a process of exclusion I was driven back to the water. I had one of the manholes taken off one of the tanks, a bucket sunk to the bottom, tilted over and dragged along ; I then found that there appeared to be some sediment. With some reluctance—for one does not like to waste good drinking water in Bathurst—I gave instructions that the water should be run off, and I found at the bottom a deposit of greenish-grey mud, in which I observed some spindle-shaped bodies, about the size and shape of oats, but

slightly longer, which gave me the key to the problem. I admit that I should not have known what these things were, unless I had seen them under different conditions a short time previously.

When riding to the hospital one morning I noticed on the horizon a long, narrow dark cloud. By about 12 o'clock the cloud had reached the town, and a dense mass of locusts was passing over, and millions were settling on the ground, on the trees, and on the roofs of the houses. And when in about thirty-six hours the visitation passed on, every blade of grass and every green leaf, with the exception of a few noxious weeds, had disappeared, and the ground was as brown and bare as one of the streets of London. But as the locusts ate, they had necessarily to defecate, and their stools consisted of spindle-shaped bodies, which were composed of the fibrous, indigestible parts of the grass matted together, and also of the silicious spicules which are to be found in many grasses. I had noticed them lying about after the locusts had gone and had examined them, and it was quite clear to me that those which had been deposited on the large expanse of barrack roof had become partly disintegrated, and had been washed into the tanks with the first shower of rain. The specific gravity of these particles kept them in suspension for some time, so that they sank slowly, and it was evident that I had to deal with a case of irritant mechanical poisoning caused by the passage of these spicules through the alimentary canal, similar to, but not so severe as, the ground glass poisoning of the Obeah Man in the West Indies (or the diamond dust of the Egyptians).

That this view was correct was shown by the sequel. I stopped the source of water supply, and utilised a surface well in the barrack-yard, the water from which was carefully boiled. The roof and the gullies were cleaned, and a considerable quantity of similar *débris* was found. The

tanks were emptied and cleaned, and the pipes disconnected until after the next heavy shower, and the outbreak of dysentery ceased as abruptly as it had begun.

It is an interesting illustration of the curious and unexpected problems of sanitation with which one has occasionally to deal in the Tropics, and the obvious lesson is, that in localities where the water supply is dependent on the collection of rain-water, and where locusts occasionally pay a visit, we should see that the pipes are disconnected until the excretory efforts of these insects have been removed. It appeared to be of sufficient interest to bring before this Society.

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## Discussion.

Mr. J. Cantlie, in opening the discussion, said he was unable to give them any information about locusts, but the irritation of the intestines referred to in the paper seemed to be related to the theory that mica in water might cause Hill diarrhœa. Possibly there was a parallel between the two cases, although many authorities did not believe that mica had anything to do with the causation of diarrhœa. Still, it was evident that the irritation which Dr. Prout had described was very like the irritation seen in Hill diarrhœa in India. He thought Dr. Hartigan could state some facts about a similar disorder which was formerly prevalent in Hong Kong, and which was also due to water contamination by mica.

Dr. Hartigan was afraid he could not add anything to what Mr. Cantlie had said, except to state that in Hong Kong cases of diarrhœa very often occurred which could be traced to nothing else except intestinal irritation from a sediment in the water. Under ordinary circumstances the water in Hong Kong was perhaps the best in the East,

being unusually soft and pure. But in the old days, when there was a heavy flood, the water sometimes washed down considerable quantities of surface earth from the hills, and that formed a sediment, which undoubtedly caused outbreaks of diarrhœa. When the water was put under the microscope, although it was apparently clear, little crystals of mica could always be found in it. The trouble undoubtedly did not arise from chemical action, because mica was insoluble; but, nevertheless, a form of irritation was set up which closely resembled that which Dr. Prout had described, and was not unlike the so-called diamond dust, or ground glass irritation described in India. The similarity of the symptoms had struck him in listening to the paper, but he knew nothing of locusts or their effects, because he had never seen a visitation of them.

Fleet-Surgeon Collingwood enquired whether it was possible that the diarrhœa so prevalent in Bermuda was, in the same way, connected with the limewash which was used on the roofs of the houses and in the water-cisterns.

Dr. Carnegie Brown wished to say a word about the statements that had been made as to mica in water being a cause of Hill diarrhœa in India. That theory was introduced a good many years ago by Surgeon-Major Dyson, who was then stationed at Darjeeling. There had been at the time a great many cases of Hill diarrhœa, or some other form of epidemic diarrhœa there, and on the water being examined a sediment of laterite, in which many mica scales were present, was found. Laterite was found in many places in the Tropics; it was almost always present in the water of Hong Kong, Penang, and other tropical stations where the geological formation was disintegrating granite. The rainfall was heavy, and detritus was washed down the steep declivities into the water sources, and Dyson naturally attributed the particular outbreak which he

observed—and which, by the way, was an exceptional occurrence—to this cause. He regarded the mica in the sediment as being perhaps also the origin of the disorder, which was then becoming fairly well known, from Grant's description, as the Hill diarrhœa of India. There was nothing to support Dyson's view, however, and his hypothesis died a natural death. Two or three years ago at a British Association meeting Colonel Duncan revived the theory of mica contamination, and supported it from his own experience. He thought, however, that Colonel Duncan had adduced very little fresh evidence to corroborate this opinion. Nothing was before them except that Colonel Duncan had seen cases of Hill diarrhœa and that he had also found mica in the water at Hill stations. That could not be seriously advanced as evidence of the cause of a specific disease. Hill diarrhœa occurred all over India, and in many other places in and out of the Tropics, and it was a very characteristic condition. It was now known that it was not an irritation, and that mud or disintegrated granite or septic contamination had nothing to do with its origin. Mud might cause diarrhœa upon the hills, as it might cause diarrhœa in the plains, but the origin of the specific disorder Hill diarrhœa was unexplained. It was very difficult to say how acholic diarrhœa originated. The late Dr. Crombie, who had a very large experience of Hill diarrhœa, considered it to be due to a specific organism, and asserted that it frequently occurred not only in India, but in South Africa and in Europe; that though it was seen mostly in the highlands, it was also prevalent in low-lands; and that it was essentially a specific disease, denoted by white stools, wasting, and other symptoms, which were as characteristic as those of enteric fever or cholera. The interesting history of the outbreak of diarrhœa which Dr. Prout had related, pointed the moral that people who depended on

rain for drinking-water should be careful that their shoots were in working order, so that the first fall of rain was not passed into the tanks. There was a well-known arrangement by which this was effected: the first water that fell upon a roof did not enter the tanks, but, after a certain amount had fallen, a shoot automatically turned, and the tanks were then filled by water from a clear surface. He had seen that mechanism on roofs in Australia; and, though it worked well, if it was not attended to, and if there was lime, locusts' droppings, dust, or any other impurity upon the roof, the tanks would certainly be polluted. Decomposing vegetation, such as rotting leaves in the gutters, was a fertile cause of trouble, but that could also be kept clear of the cistern by automatic shoots. Apparently that very simple precaution had not been adopted at Bathurst.

The CHAIRMAN said that he thought the Fellows would desire one of the Secretaries to write and thank Dr. Prout for having initiated the discussion, and in his reply he would probably be able to answer Fleet-Surgeon Collingwood's question.

# TROPICAL LANDS AND WHITE RACES.

By Dr. T. P. MacDONALD.

(*Wednesday, April 15th*, 1908.)

Mr. Chairman and Gentlemen,—In the words of Swinburne, I would first "thank with brief thanksgiving whatever gods there be" for having created so useful an institution as the London Tropical Society, which to the pilgrim of tropology in temperate climates is an oasis in the desert to those who journey towards the "holy land" of their longings. Here the sun-worshippers, as those who have heard the call of the Tropics and felt the fulness of its life in their veins might aptly be named, may meet to strengthen each other by mutual sympathies and by exchange of opinions, convictions, and experiences; and so contribute, organically, to the advancement of tropical science, philosophy, and art.

In the second place, I have again to thank Dr. Sandwith and the Council of the Society for the privilege of reading this paper before you to-night upon so apparently abstract a subject as that of Tropical Lands and White Races.

Let me hasten to assure you, gentlemen, that if I believed this question to be one of merely abstract or perhaps academic importance you would have been spared such an infliction upon good nature. Not only do I think this question to be one of the utmost concrete importance in itself, but I consider, indeed I may speak in superlative language and say I am convinced that an understanding of the logic of tropical life is vital to the movement of tropical

medicine and momentous to the destiny of the white race, perhaps to that of humanity itself.

So mighty, indeed, has the question appeared to me, as one who has consistently and persistently given his best thoughts and energies to the problem of "how the tribes of men might prosper," that I have been driven to initiate a special organization in Australasia for the study of phases of the subject, so infinitely beyond one's individual powers.

So wide is the question, and comparatively speaking so new, that the utmost seamanship will be required to steer this paper successfully through the Scylla of your scientific criticisms and the Charybdis of your political prejudice.

Some ten years ago a rather brilliant lecture was contributed to the Roman Catholic Convention of Australia, by my friend Dr. Ahearne, of Townsville, North Queensland, upon this very subject. But as Dr. Ahearne's conclusions were most pessimistic as regards the possibility of white peoples being able to occupy the Tropics under any but aristocratic conditions, and as I could see the dreadfully destructive consequences of his logic upon the popular ideals of a democratic Australia (by which term I do not refer to political ideas of democratic Australia, but to the use of that term in its sociological meaning, viz., a nation which shall perform its own unskilled work by its own people, in conformity with instincts of race survival which I believe to be the corner-stone of European national greatness), from that moment I gave myself to the study of the question with enthusiasm.

Dr. Ahearne's logic was that mostly used by the general scientific world; his methods were inductive, his measurements of children were carefully given, but I am persuaded to believe that he allowed his deductions and conclusions to be influenced by prevalent fallacies. He believed that tropical work belonged by natural right to the coloured workman; that the coloured skin is best adapted to sustain

the heat of tropic suns; that children of white parents grew weedy and lanky, and that later generations of the the white race would rapidly degenerate and disappear from tropical lands owing to the effect of climatic conditions upon their vitality,

Admitting that there are fears which "crawl with legs" in the Tropics, I would point out that there are also hopes which fly with wings; by which metaphor I wish to strengthen my point that "It is not the climate of the Tropics, but tropical diseases alone, which are inimical to human life in tropical lands."

I fear that the all-important factors of tropical disease and tropical medicine and hygiene were unappreciated in Dr. Ahearne's lecture, and that defects in physique which admittedly existed, more especially in certain centres, were attributed to climate when in fact they were due to malaria, ankylostomiasis, and other diseases quite within the reach of recent tropical science to destroy.

The study of ankylostomiasis in particular revealed to me the striking truth that children cured of parasitic disease experience a boom of good health, however tropical the climate may be.

Parasitosis is the real evil of tropical lands; sunlight gives robust life to plant and animal; it makes children healthy, happy and beautiful, and men strong and active; the young stripling who grows, overgrows in fact, lanky and loose of limb, fills out into the proportions of a young giant in early manhood, at middle age he becomes a typical John Bull.

In Geraldton and district, North Queensland, where I practised, the climate is truly tropical; in fact, Geraldton is famous as the second wettest place on the planet. I conclude that the sea itself must hold premier place, for with sometimes 240 in. and an average of 200 in. you may readily imagine the perpetual downpour. We used to say

that the wet season lasted four months, and that then it began to rain. Heat there was also of a most trying order, and frost was quite unknown.

Malaria in all its forms flourished, and sunstroke was not infrequent among horses and men when by chance the rain-clouds took generous trips to other less favoured parts.

One would expect in such conditions to find the town and country filled with human weeds of the white race. As a preliminary investigation, immediately upon the publication of Dr. Ahearne's paper, I walked down one side of the township and up the other, weighing and measuring the business men in shops, banks, and offices. They were twenty-five all told, and their average weight was 14½ stone. The majority were not natives, but they had lived in the Tropics most of their lives. One, a native of the third generation, possessed a chest measurement of of 44 in. with 7 in. of an expansion. All were in excellent health. Turning then to the children, who had mostly all been through my hands for treatment of ankylostomiasis, I could see at a glance that if one were to judge by weight and measurement false conclusions would be arrived at; but the contrast of general activity before and after the introduction of tropical medicine and hygiene into the district provided a safer criterion as to the value of climate. Activity and beauty are the tests to apply to children; so long as children are really happy and fond of play, we may be sure they are on the high road to healthy development.

In my last paper given before this Society I pictured the listless groups of children who neither cared to eat nor play, their little lives sapped by the ravages of parasites. But to attribute those symptoms to the direct effect of climate were absurd and misleading in the extreme. Anyone visiting that district now would be surprised to see the merry groups of children riding to school at full gallop

sometimes three on a pony, to watch the boys at football or cricket or on the sports ground, and the girls in their own games quite as active. When I mention that the little boys of the town got up an amateur circus, and performed quite wonderful feats of horsemanship, I think I need not seek further for proof of vital force.

A few months ago, in Wellington, New Zealand, a splendidly-developed young fellow, with broad shoulders and straight eyes, tapped me on the shoulder. "You don't remember me?" he questioned, and I did not certainly know him, as he further explained, for one of my earliest patients in Geraldton, who had suffered from ankylostomiasis, and would have supplied most pessimistic measurements to any philosopher dealing merely with generalisations based upon incomplete data. This lad had contracted his disease of boyhood in a house which was responsible for five deaths in three families successively occupying it, previous to my advent in the district. It was with grim delight I saw it burned to the ground. How sad to think of those lost lives, when 2 oz. of thymol or eucalyptus would have saved them! It may be argued that this lad, having gone to New Zealand, owes his robust health to the climate of that country; but I can point to numbers of boys who did not leave the district, but became engineers, storekeepers, and farmers, now in perfect health, and strong as young lions.

As to that bone of contention, woman in the Tropics, I might content myself by saying what none with experience would dispute, that woman in any climate defies philosophy, spurns science, and is quite beyond the reach of art! But in my salad days of tropical experience I was lucky enough to stumble upon the truth quite by accident.

I had preached tropical hygiene, sunlight, exercise, and all the good things men love in the shape of physical life, without any appreciable effect upon the habits of womanhood in our surroundings. Then a lady told me it was no

use preaching to women; you can only influence them by initiative. Get up picnic horse races, riding parties, boating on Sundays, and journeys on horseback to adjacent districts. Make those things interesting, and a number of women who find the benefit of healthy exercise will influence others in their own way.

I pondered upon this proposition, and convinced at last of the soundness of the reasoning, I set the wheels in motion to consummate my purpose to infuse a spirit of activity in outdoor life among the women of our community.

The details of this experiment would furnish material for many romances, but suffice it to say that although I knew nothing of racehorses or horse-racing, I thought it would be useful to study the effects of climate upon animals, and my experiments were so successful that in seven years I had established a world's record of wins on the turf. Rational life to the animal was my keynote to success, and I am now in a position to table details of animal life in the Tropics, and certain rules of tropical hygiene for animals, all of which helps the great main question before us.

The general effect of my friend's plan of campaign was felt in two years; gradually the doctrine of rational life in tropical lands spread, and now girls and matrons take long journeys on horseback, perhaps 60 miles in two days, fording rivers and breasting mountains, and holding their own with men in powers of endurance, while the whole scheme of life is sensibly altered.

Girls play tennis in the tropic heat with impunity; they boat, swim, dance, and generally lead active lives. Houses are no longer shut up, so as to exclude the sun and harbour germs, while splendid health is enjoyed under those conditions.

Turning to the more democratic aspect of woman's life

in the Topics, many of the farmers' daughters may be found engaging in the noble exercise of tropicultural work. They become expert cane-cutters, which work at one time white men were supposed, from race disabilities, to be incapable of performing. One woman, a champion of her kind, challenged any man, white or coloured, to compete with her in cane-cutting. She revels in the work, and would hold her own in looks with Irish, English, or Scottish rivals.

The publication of my semi-scientific data on the subject of white people in tropical lands won for me the support of the people themselves, who knew by experience the truth of things, denied by theorists who only dealt in opinions which were more or less derived from superstitions handed down from brain to brain, or book to book. I was chosen as delegate to one of the Queensland Farmers' Congresses, held in the Tropics, that southern agriculturists might have an opportunity of personally inspecting tropicultural conditions. My task was to read a paper upon the climatic conditions of North Queensland in relation to white labour.

Therefore, it behoved me to study the whole question more fully, and to examine the literature on the subject at my command. About this time a book published by a Japanese writer upon the subject of "colourphobia" fell into my hands. In forcible language the thesis was set forth that the Japanese were a people adapted by natural selection for tropical work! The deductions to be drawn and conclusions to be made from this startling premiss are obvious. The book was freely quoted by the opponents of white Australia in the Federal Parliament with telling effect, and for some time it appeared as if the logic of coloured skin for the Tropics would turn the tide of public opinion in its favour.

Going into the matter very patiently, I went beneath

the surface and enquired into the climatic conditions of Japan to find out, if possible, how Nature had adapted, so to speak, the Japanese people to hot climates. The first staggering blow to this assumption comes from the fact that the average temperature of Japan is the same as that of England, viz., 55 deg. F. Not a fraction of Japan extends into the Tropics. Furthermore, how should it happen that China, a country whose average temperature is some four degrees higher than that of Japan, should produce a people with some ten degrees lighter-coloured skin.

These observations raised the whole question of colour origin and colour distribution.

If colour is produced by adaptation to sunlight and other climatic conditions, why are the Esquimaux brown in their Arctic environment, the Finlanders dusky, the Indians red, the Maoris black, the Galla natives on the Line white, the Line Islanders most pale of Melanesian races, the Fuegans black ?

Distribution of colour proves conclusively that we must seek some other explanation than climatic conditions for the origin of colour in the human skin ?

Reclus and other geographers are correct in exploding the old fallacy that pigmentation is caused by sunlight, but a satisfactory theory as to the origin of coloured skin has yet to be formulated.

I am forced to conclude that pigmentation of the skin corresponds to the place in time of the race ; that blackest skins are found in most primitive peoples, and that between the coal-black skin of the chimpanzee and that of the white race we have a spectrum of colour, according to degrees of lightness in shade, correctly indicating the age of the race. Anatomy and sociology and colour correspond harmonically to prove my theory. Pigment is dissolved by time out of the human skin, and climate plays no part in the operation. Sunburn is quite another matter—it is due

to drying up of the superficial epithelium; pigmentation has to do with deep layers of the dermis.

One word, one argument only to prove the intellectual superiority of the white race, which it is the fashion to decry, perhaps justly, on account of its sins; for truly, as Shakespeare says, "Lilies when they fester stink far more than weeds," and white people who depart from the ideals of progress are relatively more culpable than their younger brothers.

The white race has only been in the Tropics a few years, but they have done in that time one great feat: through Bancroft, Manson, Ross, they have conquered malaria.

To prevent misconception, let me emphatically declare that I believe in the social equality of all nations and of all men. If we, the members of the white race, are older in time, we are only elder brothers of one great human family.

After my Queensland paper I was sent further afield by those who believed I had broken the logic of colourphobia and of Dr. Ahearne.

In Sydney, Melbourne, and Wellington I found opportunities of further study of books upon the colour question, but original research alone can settle it. The work will be difficult, as the skin is the most trying tissue in the whole body to deal with microscopically, but it must be done, and the sooner the better.

Whatever objections be raised against my theoretical exposition of the case in favour of the white race in tropical lands, the arguments from history, recent history, are quite unanswerable. In Australia opposition to the main contention of the people that they could and would perform all work of a tropicultural nature in Queensland has disappeared; the question is a thing of the past, and a deep thrill of hope has gone through the land that the Anglo-

Saxon people will reach the culmination of its destiny in tropical lands.

SUGAR GROWN BY WHITE LABOUR IN QUEENSLAND.

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|---|---|---|---|
| 1903 ... ... ... | 221,776 tons. | Bonus | £50,454 |
| 1904 ... ... ... | 379,884 „ | „ | 85,211 |
| 1905 ... ... ... | 500,910 „ | „ | 111,550 |
| 1906 ... ... ... | 1,197,435 „ | „ | 272,038 |

One of the historical experiments was made by a hard-headed Scotsman named Fraser. Having a huge contract to supply firewood to the sugar mills, he bethought him of putting gangs of different nationalities against each other. Accordingly gangs were formed of white men, Chinese, Japanese, Kanakas respectively. As the weather became hotter and hotter the tally of the white workers rose above their competitors, although all worked upon the same scale of sub-contract.

When the white gangs of cane-cutters commenced their pioneering experiments, I offered to attend them upon merely nominal terms and admitted their sick into my own private hospital at cost price of food, that I might secure what I thought would be valuable data. Instead of suffering great sickness, sunstroke, heatstroke, etc., the healthy tone of the gangs became quite remarkable. One man, sent by his medical adviser to recruit his shattered health on the coast from the tableland, took to cane-cutting and recovered in a month or two.

Not only are the white people of the North Queensland coast flourishing, I have travelled from Townsville to the Gulf of Carpentaria in summer months through temperatures vibrating about 118 deg. F., and a healthier lot of people it would be hard to find. During my four months' journey I saw very few cases of sickness, and Adam Lindsay Gordon's picture of the "sturdy station children" well describes the rising generation in those regions.

Even apart from recent ideas as to the destruction of

larvæ and mosquitoes, malaria shows a tendency to die out in settled districts of the Australian Tropics. I have noticed that dragon flies play an interesting part in the destruction of the mosquito; they form in groups of from six to a dozen round mobs of the insects, driving through them, changing sides, like children playing the old school game of "prisoner's base." This process I have watched for hours.

It is sufficient to merely allude to the reclamation work at Panama, to the West Coast of Africa, to the work in Central Africa in connection with sleeping sickness, and to to the general health conditions of white people in South Africa, to establish further arguments in favour of the point that tropical disease, and not tropical climate, is the enemy of white races in tropical lands.

Conditions of India may be urged as opposed to my general conclusions, but we must refuse to accept any form of purely aristocratic settlement as an argument against white race occupation. The lives usually led by white merchants and professsonal men in any country are not really consistent with hygienic principles. The life-giving force derived from manual labour is the principle we must emphasize in forming opinions as to the future of our race in tropical lands.

In the Argentine, where tropical disease is not much in evidence the white races are flourishing. Five years' history of white people at work in tropical Australia is, of course, the most valuable data we possess on this question.

If the Australian movement is copied into other British and European possessions in the Tropics, it follows that the science of tropical medicine must enter a period of rapid expansion. The direct and reflex effect upon our institutions of medical teaching must be enormous. The demand for medical men versed in tropical knowledge must be great, and means of supplying this demand must be considered,

and to complete my subject I have formulated my ideas as to the correlative development of teaching in tropical medicine likely to eventuate in Great Britain.

Our difficulties as to funds for enlarging, say, the London Tropical School will disappear as soon as the Goverment, the Chambers of Commerce, Guilds, Trade Unions, and other social institutions, as well as interested individuals, know what it means to have a population in the Tropics living at a hundred fold higher consuming ratio than that of the ordinary Eastern races. England's commerce, great as it is, would be doubled in a very few years. Land in the Tropics worth now £1 per acre would jump to £30 or £40 per acre. These are not fancies, but facts which have happened in Queensland quite recently.

In Australia business men opposed the white Australian ideals until they became convinced of their commercial value, when the movement boomed. With unlimited funds and practically unlimited supply of students of tropical medicine, the London Tropical School might easily rival in size an ordinary University, for I believe the other medical schools will be forced to incorporate systematic teaching of tropical medicine in the undergraduate curriculum, and so abandon all ideas of post-graduate teaching, as seems only reasonable from practical considerations. Rival post-graduate schools now springing up in Liverpool, Edinburgh, and Glasgow will change into undergraduate teaching centres under the attraction of useful paying propositions. Complete harmony should, in this way, eventuate, and the London School will then become the centre of general initiative, and get in touch with Universities throughout the British Empire in thorough-going fashion.

In such an institution as I imagine not only possible but probable of realisation, tropical science and philosophy will receive that attention which is at present denied them

either in London or Liverpool. Harmony is the soul of Science if it is the "life of trade," which last proverb many wise thinkers repudiate.

It may be thought that I exaggerate the importance of tropical medicine and deal too largely in fanciful theories, but I would remind you, gentlemen, that I come from the land where East and West have had their first skirmish in logical debate, where problems are growing faster than Europe can follow, and where magnificent needs call out lustily for generous supply.

I frankly admit that, if the white race fails to survive the test of further experiences in tropical lands, my whole case for reconstruction in our medical schools falls to the ground. Japan is now quite capable of rising to the occasion of race supremacy if she falls heir to those gardens of Eden in the East.

I feel happier in having shifted my burden of anxiety about this racial question from my own consciousness to yours, and my prejudices in favour of tropical lands for white races are not so deeply rooted but that they could be altered by reason. However much one may love the sun and love truth in those regions, monopoly is impossible, and I have only to add that it has given me great pleasure to prepare this paper, and I shall be still more pleased to hear it criticised. In my first cast of this paper I here extended the question into a criticism of the sociological side of the race question, but I found I should either have to say too little or too much, and so I content myself with the thought that if I have succeeded in interesting you to-night, perhaps at some future date we might, with mutual advantage, discuss the sociological side of the problem of white race survival in tropical lands.

And therefore I shall substitute for my prose conclusion a short song, not to take the edge off your criticism, but to temper it rather with emotional fire :—

WHITE AUSTRALIA.

Hail her, White Australia, hail her !
In the warm Pacific Sea,
Rising from the mists that veil her ;
Beautiful is she !
First and fairest,
First and fairest
Of the nations yet to be.

Ours not armies red with slaughter
In vainglorious war ; but we,
Every son and every daughter,
Swear that she shall be
First and fairest,
First and fairest,
First and freest of the free !

Ever fresh ideals raising,
Courage, Hope, Humanity,
Guide her by the great sun blazing
To her destiny !
First and fairest,
First and fairest,
Queen of nations yet to be !

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## DISCUSSION.

MR. CANTLIE said he had written a good deal in connection with this subject, but were Dr. Macdonald's contentions correct, he would have to retract it all. He had written from a different standpoint to that of the author, and had endeavoured to prove exactly the opposite. His picture of Australia was that of a land which had been neglected by the human race. Even Papuans had not settled in that great Continent, where there was everything necessary for their maintenance—food, climate, and so on ; and the people who had survived—the aboriginals—had degenerated to an extent that was almost unique in the history of the world. The native Australian was perhaps the lowest type of human being known. It would seem that tropical Australia was a land abhorrent to

mankind, black and white; that history had shown that even coloured people did not thrive there, and that European colonisation in Northern Australia would be unsuccessful. He had gone so far as to say that there were signs of degeneracy amongst white Australians, but Dr. Macdonald's statements seemed to refute this. One of the chief occurrences which had recently attracted attention had been the exclusion by the Australians of people who were skilled in various kinds of labour. It was well known that several hatters who went out to Australia were sent back again, simply because the people of Australia were afraid of competition coming from this country. Then with regard to reproduction, the Australians had not been a fertile race, fertility might have been expected in a people who had settled in a new country with plenty of room for expansion, but legislation and everything else pointed to the fact that they were afraid of competition, and any country that was afraid of competition was degenerate. He had consequently argued that there was very little prospect, either from a physical or racial standpoint, of success in connection with Australia. In the first place, people had avoided the country; in the second place, people who went there degenerated; and in the third place, the Australians were afraid of healthy competition from outside, and had taken refuge in their degeneracy by passing legislation which prevented skilled labour coming in. He had also pointed out that the parts of Australia which at present had the largest population were those farthest away from the Tropics; on account of heat in the north colonisation had been only active in the south, as in Canada people in that Dominion kept as far south as they could in order to get away from the cold. In the United States, however, population tended to go as far north as it could to get away from the heat; while again at the Cape all the important towns were situated in the south for the

same reason. He hoped that the picture which Dr. Macdonald had sketched would come true, that a healthy race could be reared in the north of Australia, and that it would be able to do a greater amount of work in eight hours than the people of other races in twelve. If Australia could take our surplus population and develop them into a healthier race, it would be to the benefit of both countries. He was much indebted to the author for bringing up this question, because he (Mr. Cantlie) had been, perhaps, led to some extent in a wrong direction, and he did not see any reason to doubt the figures which had been given. As the author had rightly said, the history of the third generation was the one which had to be most carefully borne in mind. When it was shown that a man of the third generation had a chest measurement of 44 in., there was very great hope for the future. When the Australian cricketers came here he had enquired into the parentage of each member of the team, and he had statistics in his possession of the parentage of each cricketer in several Australian tours. The last team were all Australian born; but most of them were the first generation, and only two or three were of the second generation. He did not think the Continent of Australia was old enough as yet to say exactly what the population would be like in the third generation. What was sometimes called the third generation was often not an accurate statement, because before one could have a third generation it was necessary that the grandfather and the grandmother, the father and the mother and the children should all be Australian born. If the grandfather or the grandmother were imported from this country, the third generation was not a correct description. The question was often asked as to where the Anglo-Saxon race was thriving best, whether at home, in Australia, the Cape, Canada, or elsewhere; whether the Anglo-Saxon race was improving by

going abroad, or whether it did better at home, and the conclusion he came to was that on the average we were developing here at least as well as anywhere else. It was true that we had lost the rowing championship to Australia and Canada, and that we had recently been beaten at cricket and at a number of other games and sports, but the ultimate test must always be in England. Whatever achievements were performed over seas, the ultimate test was the winning of a competition in this country. Could Dr. Macdonald's statements be accepted as scientifically accurate, it would be necessary for him (Mr. Cantlie) to withdraw almost all he had written on this subject.

Dr. Low enquired whether the particular part of Australia with which Dr. Macdonald dealt in the paper was tropical or sub-tropical.

Dr. Macdonald stated that the great bulk of Australia was sub-tropical, but the part dealt with in the paper was in the Tropics.

Dr. Low said most of his experience had been with regard to the possibilities of white men in the equatorial tropics of Africa, and it could not be said that they thrived there. Disease undoubtedly played its part, but he did not think it played the entire part. A parallel instance was that of certain plants and animals which lived and multiplied abundantly in the Tropics, but if they were brought to this country they quickly died, unless they were kept in conditions exactly similar to those to which they were accustomed. He admitted that many of them died of disease. Monkeys died of tubercle, a disease of cold climates, as white men when they went to the Tropics might die of malaria. Still, he did not think the white man was suitable for permanent settlement and procreative

purposes in a tropical climate. His general experience of people who had been bred and reared in the Tropics—which he admitted had been gained without close study or statistical measurements—was a distinct impression that they were not such a good breed as the home product. Most of them were, it was true, only of the first generation, but he was convinced that even in the first generation they had begun to degenerate. The best example of permanent European settlement in the Tropics was that of the so-called "poor whites" in Barbados, whose ancestors were sent out to the plantations at the time of the rebellion, in 1745 to 1750. A great number of them had kept a pure stock, and had now arrived at, probably, the tenth generation, without native admixture. A noticeable feature in them was that the hair had mostly become sandy-coloured, that they had a peculiar unhealthy appearance, and that they had unnaturally pale blue eyes and freckly skins. One would never mistake them for people bred at home. It was quite true there was no malaria in Barbados, but other diseases were prevalent. Ankylostomiasis, for instance, although not very common, was an important factor. Then, again, with regard to domestic animals, English sheep if sent to the Tropics were metamorphosed into creatures that one could scarcely recognize as sheep. The woolly fleece changed into a sort of sparse hair, unlike anything we know, and the animal degenerated in size and in everything else. To return to the human species, why did Government servants on the west coast of Africa have to get leave every twelve months to come home and recuperate? Why were they not fit to serve even another twelve months if the Tropics were a health resort for Europeans? Why could white women not live there? He admitted that there were a few, but healthy life for the majority was impossible. He knew from his own experience of the climate that there was something in it

which was terribly enervating. On first going out to the Tropics one was full of energy, but at the end of six or twelve months he was a different individual and his disinclination for work was excessive. All old residents had a species of tropical lethargy. He had in his mind a man from the Tropical School who went to British Guiana and worked at first most energetically ; after he had been there six months his letters were not quite so enthusiastic, and within a year he wrote confessing that he had fallen under the tropical influence. He did not get malaria or any other disease, he was simply a different man at the end of his twelve months' stay. The truth was, that long residence in the Tropics made white people lazy. In his opinion it was impossible for one brought up here to do the amount of work in a hot climate that he could in the temperate climate of England. In this country we did not have a siesta of three hours in the middle of the day, in the Tropics almost everybody did so. In Italy, a sub-tropical climate, during the hottest months, the whole country went to sleep from 12 to 3 o'clock. It could not be said that the Italians were a degenerate race—far from it; and the history of the Romans, who presumably lived in the same climate, and also of the Greeks, showed what could be done there. He did not think he would ever be convinced that white people could settle on the west coast of Africa and breed healthy stock. The third, fourth, and fifth generations would fail, or at least would be degenerate and useless. A good deal had been said about tropical medicine making the Tropics inhabitable by white people, but even if disease was stamped out altogether it would be impossible for white people to successfully propagate their kind there. The author had instanced white men cutting sugar cane in the fields under a tropical sun, and had stated that the third generation were splendid specimens of humanity. There might be something different in that part of the

world, but in the equatorial Tropics, of which he had experience, white men could not do so.

Dr. Hartigan thought that if the condition of things existed in Queensland which the author had described, everybody ought to emigrate there as soon as they possibly could. Personally, he had been for twenty-six or twenty-seven years in practice in China, and nobody was more keen than he was on outdoor exercise. He thought there could be no mistake about its beneficial influence in the Tropics. But there were limitations. Children were a good example of how a generation was thriving, and if they were left long in the fresh air, or if they played in the sun, the result was that they very soon became ill. They did not get sunstroke, or malaria, or diarrhœa; they simply deteriorated. Disease incidence amongst British soldiers in Hong Kong was high, and it was almost exclusively the conditions of the Tropics which produced that disease. It was not malaria, or malarial cachexia, or any specific disease. In a great measure it was simply degeneration of the vital system, due, in his opinion, to great tropical heat and damp. He did not think it was the case that the inefficiency of the Tropics was solely a result of disease. There were other influences at work. It was well known, for instance, that soldiers in the Tropics frequently drank more than was good for them, and that drink was much more prejudicial to them there than at home. Must not that be due to some inherent condition of the Tropics which could not be distinctly classified as disease? The progenitors of the poor whites to whom Dr. Low had referred, when they were sent out from England, had died like flies, although their masters survived. Both were of the same race, and living under the same conditions, except that one class was exposed all day long to the sun of the Tropics, and the other took care of themselves. He certainly could not agree

with the author that all deterioration in the Tropics was due to actual disease.

COLONEL OSWALD BAKER asked whether the children of Queensland were healthy and rosy, like the children in England and the European children seen in the hill stations of India. White children in the plains of India were always pallid, weedy, and unhealthy looking; but if they were sent to the Hills, and left for a few months, they came back rosy and healthy, and they then resembled in every way children at home.

MR. E. A. AUSTEN said that as a layman, and one who had, unfortunately, but small experience of the Tropics, he felt some diffidence in joining in the discussion; but as a naturalist, he wished to point out that every existing species of animal and plant was the result of adaptation to conditions, whether of climate, environment, or whatever it might be, which had been continued through countless ages. He had heard, during the course of the evening, that the third generation was the crucial test. But as man was a species of animal, he could not, as a naturalist, believe that perfect adaptation could be attained in three generations, either in a man, in a sheep, or in a jelly-fish. If that was possible, man would have a right to consider himself beyond the laws which regulated the animal kingdom, and no one had so far claimed that. He quite agreed with Dr. Hartigan that there were Tropics and Tropics. It was well known that dry heat, like dry cold, was much more supportable than wet heat or wet cold. Dr. Macdonald had lived in one of the wettest parts of the Tropics, although a very large part of tropical Australia had an exceedingly dry climate, and obviously there was no reason why, apart from actual disease, that portion should not be fairly healthy. He had no experience of Australia, but his experience in other parts of the tropical

world, and especially in one of the wettest parts, equatorial Brazil on the Amazon, was that so far as the white indigenous population, which were of Portuguese stock, were concerned, he had never seen more anæmic, unhealthy-looking people. They were as far removed from the picture which the author had painted as it was possible to be. It was not surprising, however, because quite apart from any question of disease, whether malaria or yellow fever (there was not much malaria, although there was a good deal of yellow fever), the climate was such that any ordinary physical exertion immediately subjected a European to the unpleasantness of constant perspiration. He knew that from personal experience, because he was engaged in pursuits which rendered it necessary that he should be fairly active all his time, and the only time during the day when he was comfortable was for ten minutes after having a "tub." It was not possible that that could be a healthy environment. That the Tropics had improved in health conditions needed no demonstration nowadays; an instance of that fact was to be found in the circumstance that, at the time of the foundation of the settlement of Sierra Leone in 1794, when it was customary to send British troops to the Tropics without much reference to climate, of one English battalion that went out 1,100 strong, no fewer than 650 men died in one year. It was inconceivable nowadays that anything like that rate of mortality could be paralleled in any battalion which was sent abroad. Obviously things had improved; but that those parts of the Tropics, at least where there was a damp as well as a hot climate, could become the future home of a white race was extremely problematical.

THE CHAIRMAN (Dr. F. M. SANDWITH) hoped Dr. Macdonald in his reply would be able to say something with regard to the exceptional droughts which many people

associated with Australia. It was refreshing to hear of the excessive rainfall described in Dr. Macdonald's paper. He had not been in Australia himself, but he had many recollections of good stories about the dearth of water. Perhaps the author could state whether there was any truth in the classical tale of a man who, after travelling in the Australian interior, eventually reached what might be called an inn. Having satisfied his thirst, he asked for a bath, whereupon, after consideration, the landlord replied, that he could have water to wash in if he liked, but he thought it only right to tell the traveller that if he preferred it he could have beer, because both were the same price! With reference to soldiers abroad, it had always been thought, in the parts of the world where he had been, that one of the chief reasons why British soldiers degenerated was that they did not have proper amusement. A good deal depended on the energy and enterprise of the colonel, and whether he induced the men to play games at proper hours: in his opinion, for instance, white men ought not to play football under a tropical sun. He was afraid that in the Tropics the men missed their usual amusements; perhaps some of them missed opportunities they had in this country of enjoying female society, but it was certainly true that they got into bad habits, and that drink, bad food, and general want of occupation were exceptionally prejudicial. The food question, which was a most important one, had not been touched upon. White men took their white stomachs with them wherever they went, and if food prepared for them was uneatable, it would take more than a generation to get accustomed to it. He had been pleased to hear the question put from the naturalist's point of view by an expert. He could not himself speak as a naturalist, but he could speak with some little experience as a gardener. If flowers were taken from this country to a sub-tropical country, or even a tropical one, occasional successes and

occasional failures would be experienced. If roses, for which England was so famous, were taken to Egypt or Turkey, most successful results would be obtained, very much better flowers being grown there than any he had seen in Europe. That seemed to indicate that roses had not lost their powers of re-adaptation, when they were taken back to countries from which they originally came. Roses were not indigenous to this country, their home was in Persia, Turkey, and other parts of Asia and Africa, where they had figured so largely in history and poetry. Ordinary English biennials would flourish in Egypt for the first year, and the grower could take prizes at the Show; but in the second year they had lost their colour, and if the gardener bred from them they would eventually all become white. In this country flowers became yellow or white when they lost pigment and vitality. In Egypt they became, as it were, anæmic, the beautiful blues and reds disappearing, and finally nothing but white remained. He could not help thinking that that condition had some relation to the anæmia of hot countries. Tropical anæmia occurred quite apart from malaria and ankylostomiasis; there were many parts of the Tropics where there was practically no malaria or ankylostomiasis, and where no known pathological cause of anæmia could be found, and yet people who lived there all became anæmic.

Dr. Macdonald, in reply, thanked the Fellows for their very generous criticism of his paper on this most important subject. Either he was very much mistaken or the Fellows who had spoken were; and if he was mistaken, then the whole Continent of Australia was so also. He thought, however, he could clear away some of what he believed to be fallacies which had been put forward in the course of the discussion. The subject was an entirely new one, but all the facts he had cited had been obtained from actual

experience. The instance of white men successfully acting as cane-cutters in Queensland was of special significance. That strength could be derived from manual work in the Tropics was one of the chief points which he desired to make, and that fact was the most important element of successful settlement. A good deal had been said about soldiers; the English had had to fight in every climate, and had generally conquered. The British Army had not been seriously prejudiced by climate: whatever the climate was, if there was no disease it did not seem to matter. It would be remembered that 10,000 French soldiers had died in Madagascar; that was from malaria, not climate. It was, moreover, impossible to overestimate the importance of parasitology in tropical climates, but he thought Australian experience exploded for ever the idea that tropical climates were in themselves inimical to the white race. White races happened to live in temperate climates, but that did not prove that they were specially and only adapted to them. Why, for instance, had the Esquimaux brown skins, and why were Tierra del Fuegans, in the Antarctic, absolutely black? The colour of the skin had absolutely nothing to do with adaptability to climatic conditions. He granted that a certain amount of acclimatisation was necessary. If people changed their residence from the Tropics to the temperate climates or *vice versa*, acclimatisation was essential, but the change was really only a small one. Personally, he had conducted a medical practice for fifteen years in the hottest part of Northern Queensland, in a wet district, and he had worked without discomfort all the time. From there he went straight to the Southern Alps of New Zealand, and was surprised to find that he withstood the cold far better than the New Zealanders themselves. His health did not suffer in the very slightest from that sudden change. His brother had been practising in Queensland for twenty-five years, having scarcely been out of the country, and he

had a family of seven or eight children, all of whom were about 6 ft. high, and of very fine physique. There was a German family in the same district, consisting of numerous individuals, all of whom weighed between 11 and 15 stone each, the average being 12 stone. He believed that when things were better understood the human race would flourish enormously in the light and heat of the sun, but the effect of tropical disease must first be neutralised. Dr. Sandwith knew that grass would not grow in Egypt, but it grew in many other tropical and sub-tropical lands, so that it was unwise to base conclusions on the analogy of natural history. There were things in natural history which were beyond the understanding even of naturalists. We were slowly arriving at the truth with regard to heredity and adaptation, but it would be rash to reason by analogy ; let us rather stick to facts. He accepted Dr. Low's statement's about the poor whites, but he should very much like to have an opportunity of examining them, for he would be surprised if he did not find ankylostomiasis. The reason the aboriginal population of Australia never progressed was apparent. Degeneracy was not due to want of food, but to want of ideas. It was ideas which made civilization. Ideas were required to develop a people. It was true that when telegraph lines were first erected in Australia, the natives got an idea; they took the wire and hammered it into spear points, but that was the most brilliant inspiration he had heard of. There were other causes, however. He believed that ankylostomiasis played a prominent part in the degeneracy of Australian natives, and although he had not had an opportunity of investigating the subject completely, there was sufficient evidence to show that ankylostome infection was prevalent among them, and he was inclined to believe it was there before the white man went to the country. There was no question that a man was healthier, was better, and happier when he

had plenty of sunlight. When disease was neutralized the beauty of the people was marvellous. Races were growing up in Australia which would surprise the world. In the course of fifteen years he had noticed a change in the racial cast of features from Brisbane right up to the north, and the further north one went, the better looking were the people. The question of Australian labour, to which Mr. Cantlie had referred, was more a political than a medical one. The democratic movement in Australia was hampered by the fact that, whenever the people made a demonstration in favour of what they believed to be their rights, they were beaten by the capitalists, who imported blacklegs from Europe under the title of indentured labour. A movement was therefore started by the democrats to exclude indentured labour, but they had no intention of excluding free labour. Contracts could not be made in Europe, that was all. Their friends, the hatters, were indentured labourers. Even then they might have come in. There was a special clause that indentured labourers could enter Australia, provided it was testified that similar labour could not be procured in the country. The employer who brought over the hatters had to certify that they were specialists, and could not be replaced, but he refused to do so, and the result was that he obtained a big advertisement at the expense of the Australian reputation. With regard to Brisbane and the Gulf of Carpentaria, let him explain that geographically these were different countries. The coast of Queensland, in fact all the east side of Australia, was a range of mountains which never receded more than 100 miles from the sea, and on the top of the mountains a tableland began which gradually sloped for hundreds and thousands of miles to the ocean on the other side. It was on that tableland that droughts occurred. Heavy rainfall was practically confined to the northern tropics of Queensland, comparatively limited in area, but

nevertheless the people were just as healthy there as on the tableland. There was little sickness among the stock-men and their children. The people had few cares or sorrows; they were jolly and happy, and some of the greatest athletes had come from those parts. With regard to the effect of sun upon plants, everything in the Tropics grew to size. Even parasites were big, and that was why they were so formidable. There were mighty fig trees that would cover a regiment of soldiers; stimulation of all sorts came from the sun. People were struggling in this country because they had been placed by fate in a temperate climate; it was their native land; but the Tropics were really the true paradise, and he believed one day the white races would wake up, make for the Tropics and never come back again.

# THE USE OF QUININE DURING PREGNANCY, LABOUR, AND THE PUERPERIUM.

By Dr. J. PRESTON MAXWELL, of Amoy.

(*Read for the Author by Dr. Hartigan, May* 15, 1908.)

Working, as the writer has the privilege of doing, in a region which in parts is intensely malarious, the above subject is frequently brought to his notice. And as there still seems to be, in the minds of many practitioners, a dread of quinine during pregnancy, as that of a drug the use of which involves considerable risk to the patient, it is well that the facts of the case should be carefully reconsidered.

It may first be laid down as a fact that admits of no doubt, that a vast number of abortions and of deaths of the fœtus before labour are due in this region to malarial fever. The exact way in which the poison acts is probably not the same in all cases. In some, hæmorrhage seems to take place in the ovum, or between the uterine wall and the ovum, and this is made more likely by the hæmorrhages which take place, not infrequently, in other parts in the subjects of malignant malarial fever. In other cases, the high temperature seems to be the cause, though the writer has several times seen very high temperatures without the child being in any way affected. Probably, too, a high temperature is not so dangerous in the early months of pregnancy as in the last few weeks. As to the question of the direct malarial infection of the fœtus, the writer is still sceptical. Cases have been published from time to time which seem to point in this direction, but, although living

in a region where such cases should be found, the writer has never met an undoubted case. Yet in some of the bad malignant tertian infections where the temperature is not excessive, and where there is no evidence of intra-uterine hæmorrhage, abortion takes place, and so far the cause of these abortions is obscure.

It must also be laid down as a fact that, so far, there is one drug, and one only, that can be depended on in the treatment of malarial fever, and that is quinine; and the medical man who, in a serious case of malarial fever, witholds the drug is risking the patient's life.

What action has the drug in question on the pregnant uterus of a healthy woman? In small doses it undoubtedly strengthens the slight uterine contractions normally present and causes them to become perceptible to the mother. In larger doses it tends to cause these to become prolonged and painful. Five grains of a good brand of quinine bisulphate was given to a patient on an empty stomach on going to bed. An hour after the uterus was firmly contracted and painful, this condition gradually passing off. In fact, contrary to the view that has been expressed by some writers, it appeared to act very much like a small dose of ergot. On no occasion has the drug appeared to initiate regular uterine contraction. But in the view of these observations, while the writer does not hesitate to give quinine to pregnant patients who have not got malaria, yet he gives it along with food in small doses thrice a day, and thereby obviates the unpleasant feeling caused by the strengthening of the uterine contractions. It must also be borne in mind that the brands of quinine on the market seem to differ greatly in their power.

To turn now to the question of malarial fever in pregnancy. In the year 1900 a Chinaman came to the writer from some distance to ask for some medicine for his wife who was suffering from an attack of malignant malarial

fever. In accordance with his usual custom he prescribed for her a solution of quinine dissolved in acid. The man, finding out what had been given, refused to take it, as he said that some years before he had been told by a foreign doctor that the administration of quinine to a pregnant woman caused abortion. In vain it was pointed out to him that unless the fever were checked it would cause abortion. He refused to listen and went away. The woman not only aborted as had been prophesied, but she also died without the fever abating. This case caused the attention of the writer to be specially drawn to the subject. In hospital work in this region a good many pregnant women come into one's hands for treatment, and not a few of them are in-patients and suffer from intermittent or continued malarial fever.

As has been already, stated malarial fever is a fertile source of abortions in this region. In one case known to the writer a European lady had no fewer than four abortions one after the other connected with attacks of malarial fever. When removed from the fever zone and thoroughly treated with quinine, pregnancy ran to term with no bad symptoms. The other usual causes of repeated abortions could be excluded in this case.

In the Chinese the connection between an abortion and a severe attack of malarial fever, usually due to the malignant tertian parasite, is often so manifest as to leave no doubt as to the causal connection. So clear is the writer on this point that he strongly advocates and carries out wherever possible the daily administration of quinine all through the pregnancy.

To quote some illustrative cases before discussing the matter further: Here is a patient, aged 28 (a European). During her first pregnancy she took 4 grains of quinine daily without any evil result, and with the result that she had had no attacks of fever to which she had before been

subject. Labour was natural, and the child was healthy. Just before or during labour she was infected with the parasite of malignant tertian. During her puerperium she had an attack of fever which was easily controlled. Six months later she again became pregnant, and having become careless about her quinine, she contracted malignant tertian badly, her temperature being near 105 deg. F. on one occasion. Quinine in acid was steadily administered at the rate of 30 grains a day for the first two days, and then the dose gradually reduced. She was at this time about three months pregnant. The fever was speedily controlled and there was no threatening of abnormal uterine action. From that time to the end of her pregnancy she took 4 to 6 grains daily without any mischance. Labour was natural and the puerperium uneventful.

Here is another patient, a Chinawoman. She was aged about 35, and was admitted to hospital for malarial fever, due to the malignant tertian parasite and malarial debility. She was at this time five months pregnant. Quinine (6 grains a day) was ordered, and she took this for a fortnight. At the end of this time she was complaining of stomach-ache, and as it was known that she had round-worms, a small dose of castor-oil and santonin was ordered. By an error a full dose of both drugs was given, and five hours later the writer was called over, to find her suffering from regular uterine pains, with slight hæmorrhage from the vagina. She was placed on her back, and a dose of opium and chloral hydrate administered. In a couple of hours a smaller dose of the same was given, and the quinine was stopped. The uterine action was arrested, and the abortion did not take place. A week later the quinine was recommenced, and she took 6 grains daily up to her confinement, becoming much improved in health, and the spleen, which was enlarged, disappearing beneath the ribs Labour was natural, and the child healthy.

Or take a third case. A Chinawoman, a multipara, who three months before had aborted during an attack of malarial fever, came under the writer's care in the sixth week of pregnancy, but was unwilling to carry out the daily ingestion of quinine. She, however, sent in to the hospital as soon as an attack commenced. During the pregnancy she had six attacks of fever, and on two of the occasions the malignant tertian parasite was the offender. On each occasion the prompt administration of 12 to 18 grains of the sulphate of quinine in acid for two days, followed by 6 grains a day for a week, stopped the fever, and she went to term without any mishap.

The last case which will be quoted is that of a young Chinawoman, aged 26. Otherwise healthy, she had already had one abortion during an attack of malarial fever. She was put on quinine 6 grains a day as a prophylactic, and shortly after complained of uterine pain, whereupon the dose was reduced to 4 grains a day. She took this for three months without any difficulty, and then left the district, taking with her a supply of quinine, and being instructed to send for more when this was done. This she omitted to do, and a fortnight or three weeks after the medicine was finished she got a severe attack of malarial fever, and died, abortion having previously taken place.

More cases could be quoted, but these will serve as examples. In the first case you have a patient who takes, on the one hand, quinine regularly; on the other hand, large doses on special occasions, without in the least causing uterine action, and with the greatest benefit to her general health; and in the second and third cases not only is the same true, but in both cases there had been previous abortions due to malarial fever. The fourth case is an example of the lamentable results of neglecting to take quinine. On the other side it must be acknowledged that several times the writer has deliberately administered

quinine to patients who had malarial fever, and were already suffering from regular uterine (labour pains) contractions, and in these cases he has always told the friends that it is impossible to save the pregnancy, and that the drug is given in the interests of the mother. It may be laid down as an almost invariable rule that if in a pregnant woman suffering from malarial fever uterine action has begun, the quinine will probably hasten the abortion; but that if uterine action has not begun, it will not start it, but will, on the contrary, probably be the means of saving the pregnancy.

Turning from pregnancy to labour, what is the effect of quinine administered during labour, and are there any deleterious effects which may accrue from its use at this time? The writer's experience fully bears out what has been stated by several authors, that by its administration not only are the uterine contractions made stronger, but they are prolonged, whilst at the same time their intermittency is preserved. Whether an excessive dose of quinine would not act like a dose of ergot and produce a tonic contraction is, in the writer's opinion, doubtful. In medicinal doses the contractions, though prolonged, are not truly tonic, and undoubtedly help on labour. It has also been alleged that quinine administered before and during labour tends to increase the flow of blood, and the loss in the third stage. The writer has never been able to satisfy himself that this is the case. Patients differ so widely in the amount that they lose at these times, and even the same patient in different labours tends to lose such variable amounts, that it a very difficult point to determine.

With regard to the administration of quinine during the puerperium the writer is at once constrained to state that it appears to prolong the duration of "lochia cruenta." As to four cases of which full notes were taken, in not one of

them did blood disappear till the eighth day, and in one of them not till the tenth day. In each of these cases the placenta and membranes came away entire; there was no sign of sepsis, and in one case the cessation of the quinine and the administration of a dose of ergot caused it to stop at once. It must be borne in mind in this connection that menstruation in Europeans in the Tropics or subtropical regions tends to be more profuse, and this may partially explain the increased loss.

Quinine and ergot have been administered together in half a dozen cases, of which full notes were taken. On each occasion the patient complained greatly of after pains. They were all fairly strong, healthy multiparæ, and all declared that they had never experienced anything like the amount of pain in previous labours. On the cessation of the quinine the pain ceased almost entirely. In all these cases quinine was administered as a prophylactic.

The shock of a prolonged or difficult labour is very apt to bring on an attack of malaria caused by the malignant tertian parasite, which many of these patients keep stored away for appropriate occasions. The temperature may closely simulate that of a septic case, and may begin about the third day, and although the presence of the parasite may enable one to say that the patient has malarial fever, it does not exclude the possibility of sepsis. In these cases it is sometimes difficult to obtain mastery over the fever, and for this reason, despite the pain that may be caused, it is probably better that those who are the subjects of chronic malarial infection should have some efficient doses of quinine during the first two days of the puerperium.

## Discussion.

Dr. Hartigan said that he thoroughly agreed with the author's statement as to the necessity of giving quinine in pregnancy, because most fevers tended to produce abortion, and if malarial fever was not checked it would also have that result. Typhus, small-pox, and plague, more especially the latter, produced abortion almost invariably. He had never met with the direct infection of the fœtus to which Dr. Maxwell had alluded, and in his experience infants were much less prone to malarial fever than adults. On the other hand, in children it was very common indeed. He did not agree that a large dose of quinine caused prolonged and severe labour pains. Regular, moderate doses increased them, but his experience was that if a large dose was given, instead of increasing the pain, it soothed the patient. He had looked up an old writer on the subject, Waring, who was very careful in his statements, and he made the remark that the first result of a large dose of quinine might be excitement, but the latter was invariably depression. That was the effect on his patients, and as a rule he obtained his object in checking the fever. With regard to the ecbolic action of quinine, and the story that one must not prescribe the remedy during pregnancy, that kind of statement had been handed down from generation to generation; it got into a book, and everybody repeated it and accepted it without having any real cause to do so. The Chinese in Hong-Kong believed that a large dose of quinine would certainly produce abortion; but he knew that a number of Chinese and European women there had taken quinine in huge quantities for that purpose, with absolute unsuccess. He agreed with the author that the various brands of quinine differed greatly in their effect,

and there was a still greater difference in results when quinine was given in solution, in tablet, or in powder. A 5-grain dose of quinine in solution was equal to 8 or 10 grains given as tabloids. In powder it was more easily dissolved, but in many instances the quinine which was taken as compressed tablets—more especially as pills with an indigestible coating—did not dissolve in the stomach. His experience was that santonin was a dangerous drug in such cases; it might produce abortion, and it increased labour pains. Another claim was that both quinine and ergot increased after-pains. He did not think quinine had any effect of that sort. Ergot did, of course, produce after-pains, whereas in his experience he had never known quinine do so. As to puerperal sepsis and malarial fever, he thought there could be no doubt that in many cases of malarial fever there was also septic infection, which had supervened without definite cause. It was not septicæmia or a streptococcic infection in the ordinary sense. He might add that if one wanted when prescribing quinine to be certain of having no ill effects, eu-quinine should be used. With that drug, all the advantages of quinine were obtained without any of the disadvantages, because it produced no congestion either in the intestines or in the uterus.

Dr. F. M. Sandwith thought everyone present had given quinine to a pregnant woman suffering from malaria, and abortion had taken place. It was difficult to say definitely whether the abortion was caused by the quinine or by the fever, but his own personal view was that the fever was chiefly responsible. It was well known that quinine was a poison, especially in the large doses in which it was formerly given, and was sometimes still given. It acted as a powerful depressant, both in man and the lower

animals, on the muscles of the heart, and on the cardiac ganglia. It has been stated that atropine would cause a heart which had been arrested by poisonous doses of quinine to beat again. He had no personal experience on the subject, but he thought it might be worth while to try that treatment where quinine had produced uterine contractions of a distressing nature. There was a general belief that large doses of quinine did cause uterine contraction in pregnant women, and did produce abortion. There was much evidence that quinine could not produce abortion from the healthy pregnant uterus. Pregnant cats, for instance, had been given quinine in very large doses, sufficient to cause their death, without emptying the uterus. Chirol, of Milan, some years ago gave 15 grains of quinine twice a day for two days to eight women, who were all in the eighth month of pregnancy. No result was produced; and in two other cases where it was necessary to induce abortion, 15 grains of quinine were given every day, to one woman for a week and to another for three days, without any effect, so that the labour had afterwards to be induced in the usual way. In the treatment of malaria, quinine would often prevent abortion, as Maxwell had said. When labour had actually begun, 10 grains of quinine acted as a stimulant to the uterine contractions, and was often used in America and elsewhere, not with ergot, which was unscientific, but instead of ergot in cases of uterine inertia or exhaustion. Quinine had often been found in the milk of nursing mothers, and therefore it obviously ought to be given with caution after a confinement.

Dr. Harford instanced two recent cases—knowledge of which had come to him from different parts of the world. In a letter from Dr. Cook, of Uganda, a case of malaria in

a young married English lady was related. She was pregnant and her doctor hesitated to give quinine, but as the fever did not go down he ultimately gave her 15 grains of quinine hypodermically; it reduced the temperature but produced uterine hæmorrhage. He then sent his patient to Dr. Cook, who gave her 10 grains of quinine a day in divided doses; the fever quickly passed away, and she had a natural confinement at term. A few days ago a lady from India told him she suffered severely from fever, presumably malaria, before her confinement. She had been treated by 25 to 30 grains of quinine daily, and she was told by the doctor that it might bring on a miscarriage, but it was a question of her life against the life of the child. Happily, however, in spite of these large doses, everything passed off satisfactorily, and she had a natural confinement. The fever came on again after the confinement, and she had taken 10 grains of quinine every day for two months; during the next six months the dose was reduced and the malaria had passed off completely. He was glad Dr. Maxwell had taken the line he had, because many medical men were afraid to give quinine during pregnancy, and women who suffered from malaria at that time were, in consequence, in very great danger.

THE PRESIDENT said the subject was a most important one, and the Society was indebted to the author for bringing it before them, and for the guidance he had given. He thought his views were sound. Fever was likely to end in abortion unless treated by quinine; if treated by quinine, pregnancy culminated in a successful issue; ergo, quinine always should be given in malaria in pregnant women. In the East he invariably gave quinine to women when labour set in, because soon after the birth was completed it frequently occurred that a latent malarial infection

developed. By a rule of that sort, not only was the patient but the doctor saved from serious anxiety. It was very unpleasant, after an apparently successful confinement, to be called up in the middle of the night to find the patient in a violent rigor, which might be septicæmic in origin but which was usually due to malaria. By a judicious use of quinine before confinement, and for a day or two afterwards, all anxiety and risk to the patient could be avoided.

# AFRICAN TICK FEVER,

WITH

# SPECIAL REFERENCE TO ITS CLINICAL MANIFESTATIONS.

By CHARLES F. HARFORD, M.A., M.D.

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(*Friday, June* 19, 1908.)

THE subject of African fevers is one which has always interested me, and it is one which has not received the attention it deserves. We have, perhaps, too readily assumed that the fevers to be met with on the Continent of Africa are the same as those met with in other parts of the world, and though to a large extent this is true, yet it is important that each form of fever should be carefully considered on its merits, and that the exact position which it should occupy should be settled only after the most careful consideration both of the clinical and pathological aspects of the disease. During the short time in which I served in West Africa I gave a considerable amount of attention to this subject, particularly as I was greatly impressed by the differences which were to be found in the forms of fever, presumably malarial in origin, which were to be met with on the West African coast, compared with the classical descriptions of these fevers in other parts of the world. I felt so strongly the necessity of inquiry in this matter, that I induced the Editor of the *Lancet* to institute a collective investigation into the subject of the fevers of Africa, and I had the honour to act for a time as a special commissioner of the *Lancet*. Unfortunately, the death of a colleague prevented me from completing this

investigation, but it gave me the opportunity of discussing the problems of West African fevers with a number of experienced medical men. A little later on I took as the subject of my Thesis for my M.D. degree "West African Fever," and the Thesis was devoted to a consideration of the various forms of fever to be met with in West Africa, in order, if possible, to distinguish those which were malarial in origin. Naturally at that time, now sixteen years ago, I had no knowledge of the kind of fever with which this paper deals, and even to the present time I am not aware that cases of Tick fever have been definitely identified in Northern or Southern Nigeria, or any of the British West African Colonies, which are the regions personally known to me, but seeing that the disease is widely prevalent both in the Congo Free State, in Uganda, and other parts of East Africa, it seems very probable that it may be identified in other parts of West Africa. It is certain that many cases now known to be Tick fever which occurred in East Africa were formerly classed as malaria, and it is most probable that this is the case in many other parts of Africa where Tick fever has not yet been recognized.

Seeing that the subject of my paper is the clinical aspects of this disease, I do not propose to attempt any historical sketch of the progress of our knowledge of the disease, with the difficult question of priority of discovery which would be included in such a task; but I must, as Principal of Livingstone College, refer to the remarkable fact that Dr. Livingstone, among the innumerable services which he has rendered to humanity in connection with the opening up of Africa, recognized the fact that a certain form of fever prevalent in Central Africa was conveyed by the bite of a tick, and it seems to me that what we need to-day are men with the powers of observation of a Livingstone, who will take a broad view of the problems which are still

unsolved concerning the diseases of the African continent.

Whilst, however, I do not attempt any historical review of our knowledge of Tick fever, yet it seems to me most important that this aspect of the question should be studied not only with reference to Tick fever, but other fevers of Africa, and especially blackwater fever and sleeping sickness. There cannot be any question that sleeping sickness has spread rapidly of recent years owing to the opening up of communication between different parts of Africa. Probably the same is true in the case of blackwater fever, which was at one time hardly recognized outside West Africa, but is now all too common throughout East Africa. The question must be raised as to how far Tick fever has existed, for instance, in such a territory as Uganda for many years past, and whether the increase is as rapid as it seems, or whether the increase is chiefly apparent, and is really due to the fact that it was not recognized before.

One point is quite clear, and that is that the systematic examination of the blood, which is now being carried out by many medical men in Africa, makes it possible to diagnose many diseases with certainty in a way which was not possible before. This Society has considered on various occasions the special characters of *Spirochæta duttoni*, which is generally acknowledged to be the cause of African Tick fever, and I will only venture to express my opinion that this organism belongs to the protozoa, and that whilst there are no doubt slight morphological differences between this parasite and the one usually known as *S. obermeiri*, yet these do not appear to be greater, if as great, as the differences between various forms of malarial parasites. I take it as proved that the form of fever which, for the sake of convenience, I refer to as Tick fever, is conveyed by the tick *Ornithodorus moubata*. With these preliminary re-

marks, I now pass to the consideration of the very interesting series of cases which have been most kindly supplied to me by Drs. A. R. and J. H. Cook, of the Church Missionary Society, Uganda, and I think it reflects no little credit upon these two brothers that amidst the great strain of carrying on a hospital of 120 beds, with 1,500 in-patients in the year and 90,000 out-patients, they have been able, with the assistance of some capable English nurses, to furnish the admirable series of notes of 66 cases, which I am able to put before you this evening. I may add that both the brothers are keen surgeons, and carry out every kind of surgical operation, so that they might well have been excused if they had been unable to make blood examinations, especially when it is known that the two brothers have seldom been in the hospital together, and have thus had to do their work single-handed. Of the 66 cases, 39 were males, 27 females; and again, out of the total number, 46 were adults and 20 were children. Of the total number, 57 were cured and 9 were fatal.

### Blood Examination.

Blood examination was recorded in 57 out of the 66 cases, and spirochætes were found in all these except one. In one of the cases only one spirochæte was found after a prolonged search, but in others they appear to have been fairly plentiful. In some cases, where spirochætes are said to be scanty, it was clear that the case was one of a relapse, and not of the primary paroxysm, and in some cases it was a case of a second attack of the disease. In the three cases which died within a few hours of admission, it was noted in one case that the spirochætes were "teeming," and in another "intense infection, three to four in a field." In the third they were "abundant." In others which died either on the first or second day after admission, it is only noted that spirochætes were found.

Dr. J. H. Cook notes that spirochætes are generally found in the blood during the febrile periods, disappearing in the interval. Sometimes he has been unable to find them even in a primary attack, though he has had no doubt from the symptoms that it was a case of Tick fever. He considers that it is difficult to find spirochætes after the second relapse, and in the notes appended to the cases blood examinations are not often mentioned, except during the primary attack of fever.

## Characters of Fever.

As may be expected, a large number of cases show only single paroxysms. This is partly to be accounted for by the fact that the patients recovered sufficiently to leave the hospital, and did not come back a second time. In many cases, however, it is believed that the cases were second attacks, which always seem to be greatly modified.

*Number of Paroxysms.*—There were 39 cases of one paroxysm; 12 cases of two paroxysms; 9 cases of three paroxysms; 4 cases of four paroxysms; 1 case of five paroxysms; 1 case of six paroxysms.

Next, taking the *duration of the paroxysms.*—10 lasted one day; 23 lasted two days; 30 lasted three days; 17 lasted four days; 10 lasted five days; 3 lasted six days; 6 lasted seven days; and 1 lasted thirteen days.

It will be seen, then, that under ordinary circumstances a paroxysm lasts about three days. Probably many of those given as one or two days were cases which had been ill before they came into hospital. whilst some of those which lasted for a much longer period may have been due to some complications. The interval between relapses is stated to have been one day in 4 cases; two days in 4 cases; three days in 3 cases; four days in 6 cases; five days in 2 cases; six days in 7 cases; seven days in 9 cases; eight days in 8 cases; nine days in 4 cases; ten days in 3 cases; fourteen

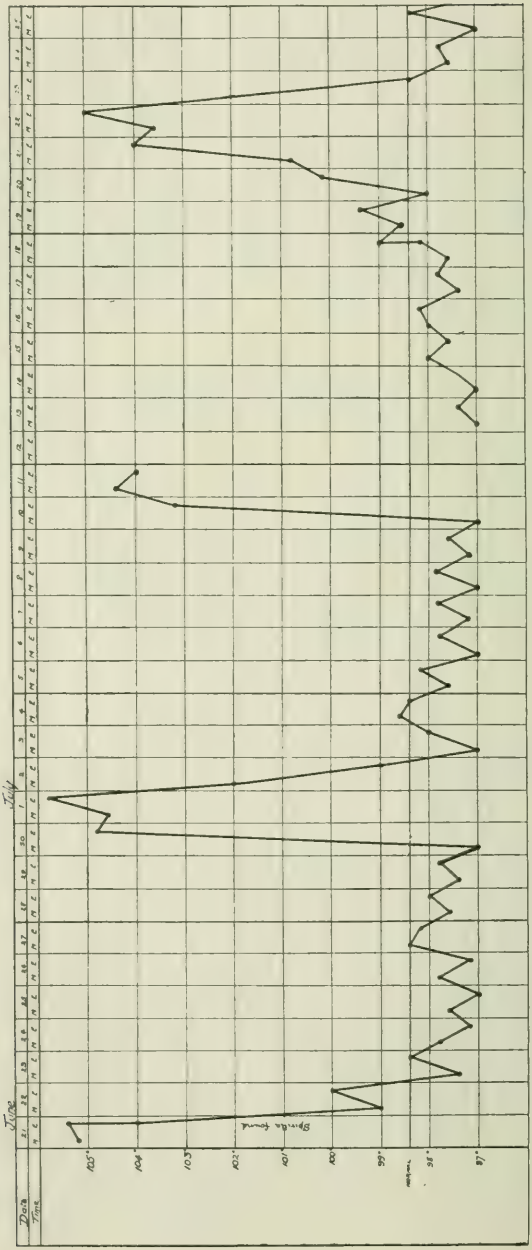


CHART 1.

days in 1 case; five weeks in 1 case (two separate times of admission).

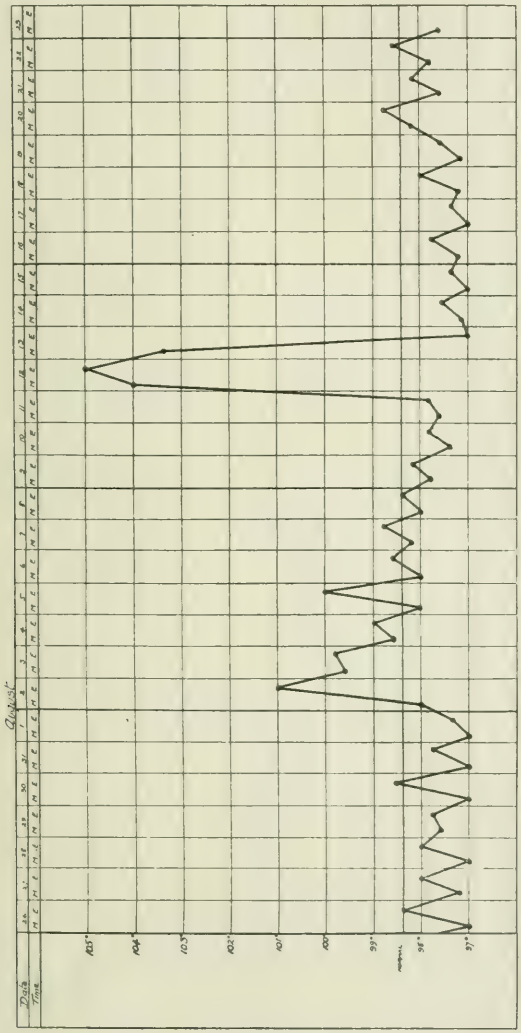

CHART 1—*continued.*

These figures show that there is a great variation in the times intervening between the relapses, though it may be taken as a general rule that the first relapse comes on

after an interval of about seven or eight days, the later ones being more irregular in their time of occurrence.

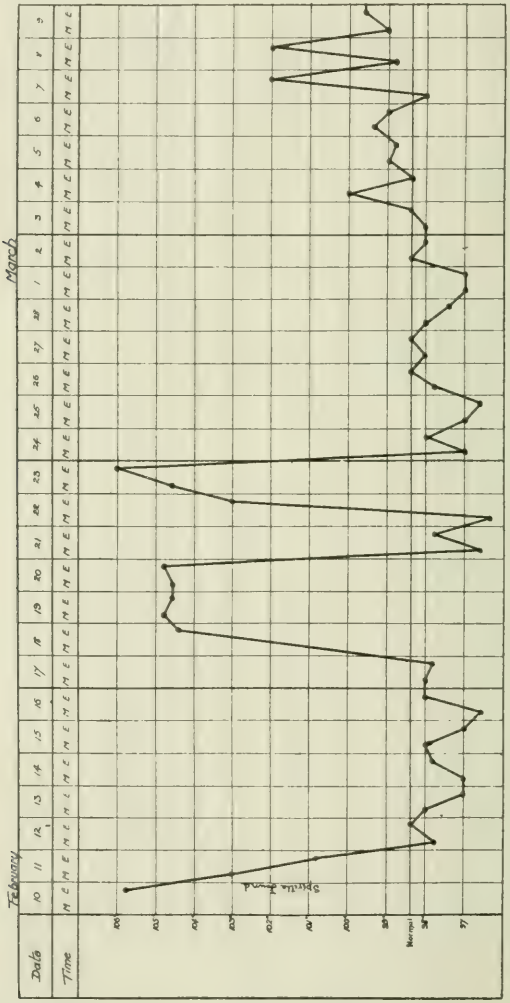


CHART 2.

Dr. J. H. Cook notes that the number of relapses is variable, on an average perhaps four or five, but he has known as many as nine or ten sometimes. He states that

sometimes without pyrexia in the later relapses there occurs, at the expected time, when the relapse is due, malaise, headache, or gastric upset. He says the usual interval is eight days, and this corresponds with the usual period of incubation. In other cases it is even longer, but not often shorter, except in cases of double infections.

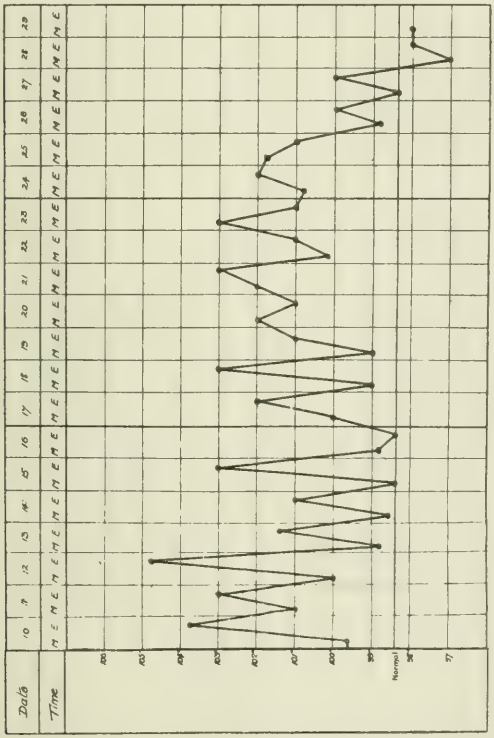


CHART 2—*continued.*

As regards the height of temperature, one striking point of the charts[1] which I am able to show you is the large number which are above 103 deg., thus 4 cases reached 106 deg. or above; 13 cases reached 105 deg. to 105.9 deg.; 17 cases reached 104 deg. to 104.9 deg.; 13 cases reached

[1] Although twenty-two charts were shown at the meeting, only four can be reproduced here. These are Charts 1 to 4 referred to in the paper, and may be taken as characteristic of the usual course of the fever.

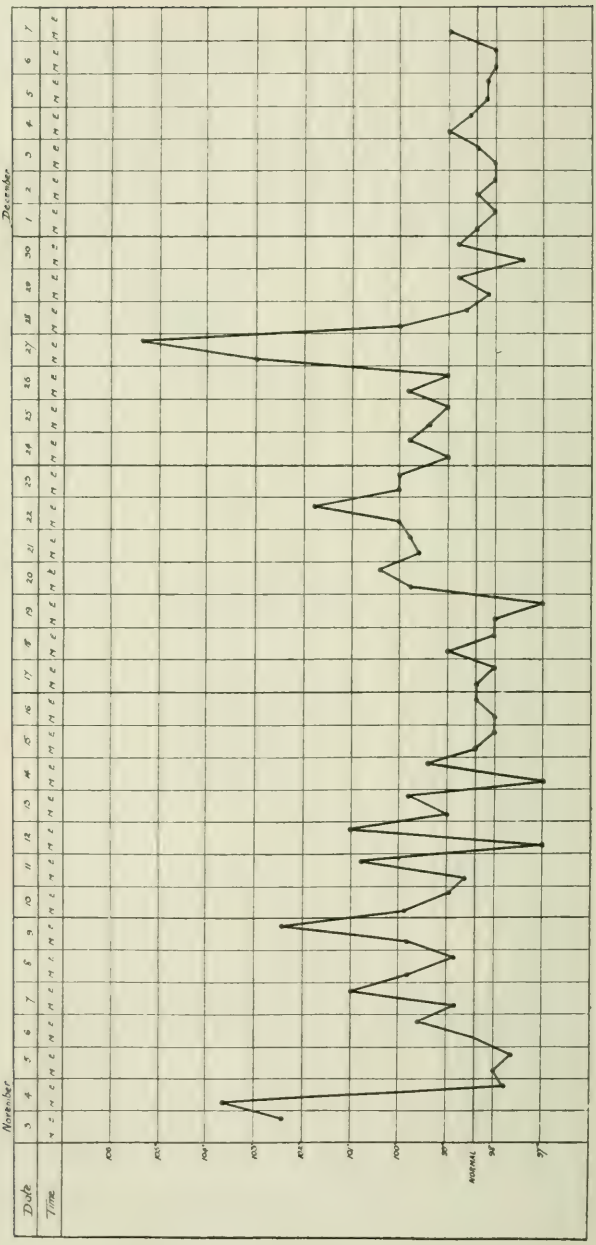


CHART 3.

103 deg. to 103.9 deg. Turning to a more detailed study of the general characteristics of the charts before you, there is one chart, Chart 1, which shows practically five relapses, and which also may be taken as the most characteristic of any of the charts, so we will select this for special attention, comparing it with the other charts. It will be noticed in this that each paroxysm shows a sudden rise, the tempera-

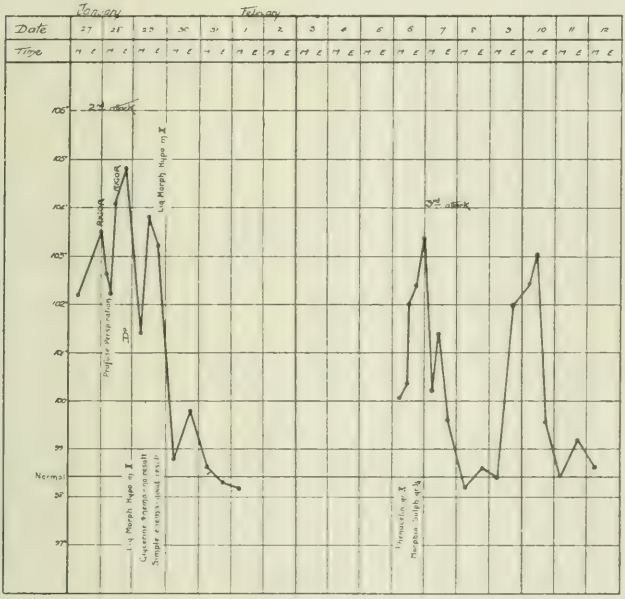


CHART 4.

ture keeping high for about twenty-four hours, usually with an intermediate drop of about ½ deg. to 2 deg. A very similar type of paroxysm will be found in Charts 5, 8, 9, 10, 14, 17, and 22. Usually in the period between the paroxysms there is complete apyrexia, but there is a peculiar rise of temperature in Chart 1, which I take to be the fourth relapse which does not follow the characteristics of the other paroxysms. In this there is a sudden rise to 101 deg., and after that more or less remittent temperature

for six days. In several other charts there is at certain stages of the fever a markedly remittent temperature with remissions of from 3 deg. to 6 deg., notably in Chart 2, and in certain portions of Chart 3; yet in neither of these cases is there any record of any special complications which could account for this peculiarity in the temperature. In Chart 16 the chart is modified by septic complications, which no doubt accounts for its character.

In some cases each paroxysm seems only to last a few hours, such as in Charts 11, 12, 13 and 20, but it might be questioned whether in these cases these paroxysms were not malarial in character. In Chart 12 it is noted that the spirochætes were scanty, there was marked leucocytosis with 45 per cent. large mononuclears. In Case 20 jaundice is mentioned as a symptom which does not appear in the other cases, so perhaps this too had some malarial complication. In some cases, in what appears to be the middle of a paroxysm, there is a sudden drop to normal with an immediate rise of about the same height as before, and this apparently without any special treatment to reduce temperature. This is seen in Charts 7 and 17. Chart 4 is stated by Dr. Cook to be a case of double infection in a European. He does not give the grounds for his opinion, but he considers that the remittent temperature during the first part of the chart, which he states was really the first relapse, was due to two broods of parasites which he considers took different times to mature, so that in what he would call the second relapse there is an interval of two days between the paroxysms due to these two broods.

In at least one patient of mine, whose case is not included in these charts, a low form of fever continued for some time after the conclusion of the relapses, but this does not seem to be a common occurrence, as shown by the charts before you, though in Charts 5 and 18 this appears to be the case.

### Other Symptoms.

As regards other symptoms, a rigor is mentioned as occurring in 21 cases. In 24 cases there was no rigor, whilst in the remaining cases the question of rigor is not referred to. It is noted that the spleen was enlarged in 35 cases, and that there was no enlargement in 20 cases, and though it is probable that most of the patients had been liable to malaria, yet it seems justifiable to regard enlargement of the spleen as a common symptom of the disease. Vomiting was noted as a prominent symptom in 30 cases, and diarrhœa in 9 cases, delirium being only referred to in 6 cases. From these figures it appears that beyond the usual symptoms of fever there is often some gastro-intestinal disturbance, but no other symptoms which can be regarded as pathognomonic of the disease.

### Complications.

As regards complications, Dr. J. H. Cook has given the following interesting notes, which must be taken in addition to the cases already referred to.

*Miscarriage* appears almost always to take place in cases of pregnancy, and two cases appear among the cases given.

*Iritis.*—Iritis is a frequent complication. It is usually plastic and very painful, and lasts about five weeks, and risk of adhesions is great. The tendency is to get absolutely well with careful treatment. He has seen three or four cases of serious irido-cyclitis from Tick fever. The prognosis here, too, is hopeful, and recovery of sight also perfect.

*Facial Paralysis* is a serious complication which he has noted in three cases. The prognosis is hopeful, and duration, as a rule, six to eight weeks.

*Blackwater Fever.*—But perhaps the most interesting of Dr. Cook's notes is his reference to the possible connection between Tick fever and blackwater fever. He has noted this in two cases. In the latter case no spirochætes were found, though the case seemed to be one of Tick fever. In neither of these cases was there at the time any sign of malarial infection. Dr. Cook raises the question whether it is possible that there may be any special relation between Tick fever and blackwater fever, such as is usually traced between blackwater fever and malaria. On the other hand, he alludes to the suggestion made by Dr. Sambon that blackwater fever may be conveyed by the bite of ticks; and he suggests that possibly the same tick communicated parasites both of Tick fever and of blackwater fever, if the latter fever can be said to be due to a distinct organism. For my own part I am inclined to think that there may be many causes for the occurrence of blackwater fever. Malignant malarial infection seems to be the primary cause, whilst many other circumstances, such as chill, or the taking of an unusual dose of quinine in those especially susceptible to its action, may be accelerating causes, and it is possible that a paroxysm of Tick fever may be the determining cause in these cases.

Too much stress must certainly not be laid upon the cases which have been quoted to support this view. In the first of these spirochætes were found, and it seems to have been a fairly characteristic case of Tick fever; but in the second, no spirochætes were found, though the evidence available, which is very full, suggests this diagnosis. This is of so great interest that I propose to put some of the facts before you.

The patient, a European clergyman, who first went to Uganda in 1887, and who is now aged 51, had previously had good health, with the exception of three serious illnesses:—1. Blackwater fever, in March, 1900, for which

he was invalided home; 2. A severe abdominal attack, stated to be gastric catarrh, possibly duodenal ulcer, in April, 1905; 3. The present illness.

This commenced on June 7, 1907, soon after he had returned from an exhausting journey in places where he would be particularly liable to tick infection. The patient describes it as an ordinary attack of fever commencing with a rigor. He had nausea, no vomiting, abdominal pain, and diarrhœa, with pain in the bowel relieved by passing a stool. He was not seen by Dr. J. H. Cook until June 9. Then there was no fever, no abdominal tenderness, and no parasites were found in the blood.

He remained well until June 23, about fifteen days after the last attack. He was then taken acutely ill, with vomiting, abdominal pain, chiefly referred to the umbilicus, no rigor, and a temperature of only 100 deg. No parasites were found. On June 24 vomiting stopped, and the temperature was normal. On June 25 vomiting recurred, stomach washed out. June 26, vomiting stopped. June 27, acute abdominal symptoms again returned, and the temperature rose to 103 deg. The temperature did not rise higher than 100.2 deg. on June 21, but on June 28, 29, and 30, the temperature rose each day to over 105 deg., with considerable remission each morning. The patient at times was much collapsed; the abdominal symptoms were severe, but there were never signs of general peritonitis. The pain on June 29 was described as of a scalding character passing from the stomach to the anus. Injections of strychnine, morphia, and quinine were given at various times, and rectal feeding was employed.

On July 2 an examination of blood was made with the following result:—

Red blood count. Erythrocytes, 5,360,000 per 1 cmm.

White blood cells. Leucocytes, 6,800 per 1 cmm.

Ratio of white to red—1 : 788 (no absolute leucocytosis).

Differential blood count.

| | | |
|---|---|---|
| Polymorphonuclear | 57.1 | Relative Lymphocytosis. |
| Lymphocytes | 34.5 | |
| Transitional | 4 | |
| Large Mononuclears | 4 | |
| Basophil | .4 | |
| Eosinophil | 0 | |
| | 100 | |

Blood film negative.

At this time the question of laparotomy was considered, but on the next day the temperature fell, and by July 9 the urgent symptoms had all passed away.

On July 24 a characteristic attack of blackwater fever occurred, running the usual course, but with severe vomiting and some recurrence of abdominal symptoms.

Early in September the patient left for England, and made a complete recovery on the way home, only slight abdominal tenderness persisting.

I am certainly inclined to agree with Dr. Cook that the case was one primarily of Tick fever, in spite of the absence of spirochætes in the circulating blood. It is possible that the blackwater fever has some direct connection with Tick fever, but I am inclined to think that, the Tick fever being the primary disease, the paroxysms of fever produced the recurrence of the abdominal symptoms and the blackwater fever, from both of which the patient had previously suffered.

## Immunity.

Dr. J. H. Cook does not believe that immunity exists either naturally or acquired by attacks. He knows many people who have had two distinct attacks, though he remarks that second attacks are usually less severe, and with fewer relapses than in the case of primary attacks.

## Conclusion.

I had hoped to have been able to institute a comparison between the relapsing fever of Africa and that of India and other parts of the world, but the amount of material which Dr. Cook has supplied to me has required so much arrangement that it has left little time to deal with other parts of the world. The charts which have been published by the Liverpool School of Tropical Medicine correspond very closely with those furnished from Uganda, and clearly describe the same disease. I should like to have commented upon a very interesting outbreak of relapsing fever reported from one of the schools of the Church Missionary Society in Nasik. This outbreak was carefully investigated by Captain Mackie, I.M.S., who believed that it was conveyed by pediculi corporis. This is a particularly interesting point, but no doubt requires further confirmation.

In conclusion, let me say that I have no doubt myself that Tick fever is practically identical with the relapsing fever of other parts. Differences there may be in the clinical features of the disease, as in the morphological characters of the parasite, but I hope that we shall dismiss the titles of Tick or spirillum fever and call the disease for the future relapsing fever.

I must express my great indebtedness to Dr. Wigram, of Peshawur, who has carried out a great part of the arrangement of Dr. Cook's statistics, also to various of my students at Livingstone College who have prepared the charts for the demonstration this evening. The facts I have ventured to put before you are, I hope, worthy of your consideration. I fear they have been set forth in a somewhat disjointed manner, and for this I hope you will accept my apology. My earnest hope is that it will stimulate discussion, and contribute in some small way to the better understanding of one of the most important diseases of Tropical Africa.

## Discussion.

The President expressed the thanks of the Society to the author for his exceedingly interesting paper, particularly as it broke almost new ground. The author had been an admirable interpreter of Dr. Cook's work in Uganda.

Dr. Sambon desired, in the first place, to endorse the President's remarks with regard to the admirable paper which had been read. Dr. Cook's numerous, accurate, and important observations gave the highest value to the communication. Personally, he had seen very few cases of relapsing fever, and his experience related to many years ago, so that he did not think he was qualified to speak on the clinical aspects of the disease; but there were one or two points with regard to its epidemiology and parasitology which he wished to discuss. In the first place, he wished to refer to the different forms of relapsing fever. It was their President who, many years ago, in examining a case from Gibraltar, suggested, on account of the large number of paroxysms (he believed there were eight), that probably there might be different types of fever due to different species of parasites. And when Drs. Dutton and Todd read their paper on African Tick fever at the 1905 meeting of the British Medical Association, he (Dr. Sambon) took the same ground. Considering the world-wide distribution of relapsing fever, the apparent clinical differences of the disease in different places, and that in each place there seemed to be a special carrier of the disease—*e.g.*, the *Ornithodoros moubata* in Africa, the *Argas persicus* in Persia, the bed-bug in Europe—he had suggested that probably a plurality of spirochæte species would be found. Since then, several authors had endeavoured to separate the African form from those of Europe and Asia, but it seemed to him

that neither the clinical distinctions nor the morphological characters of the parasites hitherto brought forward were sufficiently definite to sanction specific separation. An important fact had been demonstrated by Drs. Fulleborn and Mayer with regard to the spirochæte of fowls, which had a world-wide distribution. It was that although *A. miniatus* was the carrier of the parasite in America, and *A. persicus* was the carrier in the North of Africa and Persia, they had obtained transmission of the disease in fowls by the African tick, *O. moubata;* and they had thus shown that a species of spirochæte was not necessarily dependent for transmission on one species of carrier. With regard to the cases in which Dr. Cook said that both blackwater fever and relapsing fever occurred, suggesting that the tick might be the carrier of both infections, he could recall an interesting case published by Professor Laveran. Some ticks were sent to him from South Africa by Theiler, who said that if these young ticks were placed on cattle spirochætiasis would be produced. Laveran had the young larvæ placed on a cow at Alfort, and after the period of incubation spirochætiasis appeared in the animal, but a short time afterwards babesia also appeared, so that it was evident the two were carried by the same tick. Although he (Dr. Sambon) had no definite observations to offer on the point, he thought there was a great probability that the agent of blackwater fever was also carried by ticks, and he held the opinion that that was a form which, if it was not actually babesia, was closely allied to it.

Dr. F. M. Sandwith hoped some of the Fellows would be able to throw light on the question whether African Tick fever was the same as, or different from, relapsing fever, and whether either or both of the diseases were the same as the spirillum fever of India, about which Vandyke Carter wrote. Personally, he was acquainted only with

the relapsing fever, in which the disease was conveyed by *S. obermeieri*, and of which he had seen several hundred cases. In relapsing fever the number of paroxysms and the duration of the fever were practically identical with those of Dr. Cook. An interesting point was that in the African cases jaundice was only present in one case, whereas in Europe, Egypt, India, and elsewhere, jaundice was one of the important symptoms, it being present in about one-sixth of the cases. The double infection was quite a new idea to him. He sincerely hoped, for the sake of the human race, that Dr. Cook was not right in that respect. Although jaundice was not a necessary accompaniment, large liver and spleen were present in all cases of relapsing fever, but not to the extent that they could always be felt; while delirium and vomiting were common symptoms. He had never seen a pregnant woman who remained intact during relapsing fever, miscarriage always occurring. He did not think eye diseases were common in relapsing fever. Iritis and ulceration of the cornea are rare complications. He did not remember, either, to have seen facial paralysis. The interesting view had been put forward that Tick fever and blackwater fever, which occurred in two cases, might possibly have some mystic association. In the relapsing fever met with in Europe and North Africa there was no such thing as blackwater fever accompanying it. It was true that immunity was not acquired by a first attack of relapsing fever, because he had had several patients whom he had attended for more than one attack. With regard to the causation of relapsing fever, unless we were quite certain that the African disease was different, one must be careful about calling it Tick fever, because most of those who had studied relapsing fever thought it was a bug fever. At the beginning of 1884, when he was appointed Head of the Public Health Department in Egypt, the very first time he ever made an inspection he was told

that there was typhoid fever in a certain village. He went to the place expecting to find the water and milk polluted. He saw several patients, some of whom had a temperature of 104 deg., and there was no doubt they had fever. He was surprised, however, that all had clean tongues, and that all were constipated. At that time he thought diarrhœa was one of the necessary symptoms of typhoid fever. For a few minutes he did not know what was the matter. He then saw a man who had come in groaning with pain, and who said that he had had the same pain previously, that he had been back at his work for a week, and now the pain had come on again. Fortunately, he remembered Murchison's lectures on relapsing fever, although he had never seen a case. He found that several people in the district had had many attacks; he obtained specimens of blood, took it back to Cairo, found the *S. obermeieri* in it, and thus became certain that the disease was relapsing fever. In the prisons relapsing fever occurred practically every spring, although it was often mixed up with typhus. Almost every prison was overcrowded and dirty. The refuse was taken away, and the walls whitewashed, and it was discovered that the walls of the prison were full of bug-nests, there being several thousands in a room of moderate size. Later the walls were properly cleaned and filled up, and a year or two after those reforms were carried out relapsing fever ceased to exist in the prisons, and at the present time it was hardly ever seen. He knew there were no ticks because the prisons were constantly inspected, and if there had been any they would have been discovered, but there were crowds of bugs. Relapsing fever, however, still went on in the villages where the walls had not been treated in that way. He recognized that Tick fever and relapsing fever were different bacteriologically.

Dr. Bagshawe, speaking from his knowledge of the disease in Uganda, said that slight jaundice was a fairly common symptom in that country. He remembered one case of a baby which died in about forty-eight hours, the mother having also suffered from fatal Tick fever. The child was intensely jaundiced. He did not know at the time what the cause was, and on opening the body he found nothing in particular. But blood smears made from the liver and spleen were simply crowded with spirilla. He had been rather surprised to hear that Dr. Cook had 9 fatal cases out of 58, because with the exception of the pregnant woman and the child he had mentioned, which was not of full term, he never remembered to have seen a fatal case of Tick fever. He had seen one or two cases of people who had died, and spirochætæ were found in their blood, but death was due mainly to some other cause. He suggested that some of Dr. Cook's cases also had some other disease. For instance, a large number of people in Uganda had trypanosomiasis.

Dr. Castellani said that while spirillar fever was extremely common in some parts of India, it was very rare in Ceylon—in fact, during the four years he had been in Colombo he had seen only six cases, four of which were imported from India, while the other two had probably contracted the malady in Ceylon. One of the two cases was a Cingalese boy who had never left the island. With regard to the symptoms of the disease, he was of Dr. Sandwith's opinion, namely, that, clinically, it was almost impossible to distinguish Indian spirillar fever from the European, American, and African disease, but he thought the spirochætæ which gave rise to the disease were different species. That had been demonstrated by the work of Novy, in America; Fulleborn, in Germany; Laveran and Mesnil, in France, as well as by Todd and the other

workers in the Liverpool School of Tropical Medicine. With regard to the treatment of the disease, in four of the six cases he mentioned he used arsenical preparations, namely, atoxyl and quinine cacodylate. It was difficult to say whether recovery was due to the arsenical preparations, or was simply spontaneous; but as atoxyl, quinine cacodylate, and other arsenical preparations gave good results in diseases due to spirochætæ and other protozoa, he thought that treatment might be more generally used in human spirochætosis. It was well known that Salimbeni, Marchoux, and Levaditi had shown that atoxyl and other arsenical preparations had a very beneficial effect on the spirochætosis of fowls. He desired to repeat what Dr. Sambon had stated, namely, that it was Sir Patrick Manson who first stated that there were several different species of spirilla infecting and giving rise to the disease in men.

Dr. Low said he had seen but few cases of Tick fever, but after listening to the paper and inspecting the temperature charts he thought some of the African cases were probably double infections, not with the same parasite but very probably with malaria. He remembered that, in the *post-mortems* he made while in Uganda, almost all the natives had enlarged spleens or malarial pigment in the spleen. Manifestly, in a case of relapsing fever, if malaria was present as well, the temperature chart would be unreliable. It was excessively difficult, in dealing with natives of a place like Uganda, to get pure cases because of so many other collateral infections. Personally, he thought the two cases of blackwater fever mentioned were a mere coincidence. He did not know whether Tick fever had yet been reported from the West Coast of Africa; it was probably present there also; but in the careful examination of the blood of blackwater fever cases from

those parts he had never seen spirilla. The paper was an important one, because it emphasized the difficulty of diagnosing similar cases from Africa. He desired to ask whether the leucocytes were counted in the Tick fever cases? As far as he remembered, in the cases of relapsing fever there was a very high leucocyte count, but in the case Dr. Cook quoted there were only 6,500. It would also be interesting to know whether any of the cases had trypanosomes. That infection was very common indeed in Uganda. It would also be interesting to know how the blood was examined. Were fresh films made, or stained ones, and what length of time was taken to examine them?

Dr. C. J. BAKER remarked that he had lately been in a part of Uganda where spirillum fever was not very prevalent, but in the few cases he had seen he had noticed pulmonary symptoms, which had not been mentioned by the author. The patients had dyspnœa, and all the appearance of people suffering from pneumonia. On examination, however, no pneumonic signs were found, but there were a few râles in the back as a rule. He had never come across any fatal cases, and he did not know of any of the medical officers in the Government service who had done so. Dr. Harford had mentioned that the cases were on the increase. That was due to the fact that communications were being opened, and to the roads being infested with ticks. The rest-houses had been the means of spreading the disease to an enormous extent, and it was now extending to out-lying districts of Uganda, which had hitherto been untouched.

DR. HILL inquired whether there were any parts of the world where relapsing fever did not exist, because it seemed that, by mapping out the geographical distribution of the disease, further information might be obtained as to its cause. Dr. Sandwith had referred to bugs as

carriers of the disease. Certainly they were common in South China, but they also were common in other parts of the world where relapsing fever was not found. He would, for instance, like to know whether relapsing fever had been detected in the East End of London. At one time there was, he believed, an epidemic of relapsing fever in Ireland; why did it break out there for a time and then entirely disappear? With regard to the symptoms, he found those which had been mentioned coincided with those of the few cases he saw in China; notably jaundice, and also the fact that spirilla were present only with fever; there were no spirilla during the apyrexial stage.

THE PRESIDENT, before calling on Dr. Harford to reply, said he desired to make one or two observations of some historical interest. One of the Cooks, he forgot which, was the first to discover the spirillum in Africa, although not the first to recognise it. Many years ago, Dr. Cook wrote to him saying that he was surprised to find an enormous number of flagella, which were supposed to be malarial, in a slide of blood obtained from a patient suffering from fever. Personally, he had no doubt that that slide of blood came from a spirillar case, and not from a malarial one. Another interesting and sad historical fact was that Dutton's death supervened immediately on an attack of relapsing fever. He desired to ask the author whether anything had been done in the way of introducing the serum treatment for relapsing fever. Novy, MacNeal, and certain German and Russian investigators had shown that the apyrexial period in relapsing fever was brought about by the death of the spirilla in consequence of a toxin which they themselves developed in the blood. If the blood of a relapsing fever patient was taken during the apyrexic interval, and a blood slide containing living spirilla was treated with that blood, it was immediately fatal to the

spirilla. It had been found by experiment on the lower animals that it was possible to abort, and even to prevent, relapsing fever infections. He would like to know if that serum discovery had been applied to the treatment of relapsing fever. In reply to the question as to whether relapsing fever had occurred in the East End of London, he had seen it in three, if not four, cases; not in Englishmen, but in Indians from Bombay, who had been sent to hospital under some diagnosis other than relapsing fever, and in the course of routine blood examinations the spirilla were found. It was an important matter, therefore, for those engaged in tropical practice in England to remember the possibility of relapsing fever in patients from abroad. Assuming that an infection might last through eight, nine, or more cycles of the disease, equivalent to three, four, or five months, it was quite possible that the disease might be brought home by Europeans from Africa or India, and that fact ought not to be overlooked.

Dr. Harford, in reply, said he was very much obliged to Dr. Sandwith for having made up some of the deficiencies in the paper by the extremely interesting account of the symptoms of relapsing fever in Egypt. Although he felt he had no right to express an opinion on the subject, he had no doubt it was the same fever, and he hoped medical men would give up talking about Tick fever and speak definitely of relapsing fever. With regard to the question of the symptom of jaundice, he believed there was one other case in which jaundice was mentioned, but those were certainly the only two out of sixty-six in which it occurred. In reply to Dr. Bagshawe's question as to the high mortality in Dr. Cook's cases, he had received particulars of the deaths, from which it appeared that three of the patients died a few hours after admission, and four died on the day of admission, so that these evidently came

in in an exceedingly bad state. In one case a child died the next day. There was also the case of pregnancy, and another case which he had not been able to trace. He had pointed out that, in the fatal cases, the infection was evidently a very virulent one. With regard to the question of treatment, he did not know whether Dr. Cook had tried atoxyl, and he was not aware that the serum treatment had been used. He hoped both those suggestions, which he would certainly pass on to Dr. Cook, would be taken up. Most of the cases were, he believed, simply treated by diaphoretics and in a general way without a real hope of any treatment having much effect. With reference to what Dr. Low had said, he agreed the irregular temperatures were in many cases due to malarial complications. He did not think Dr. Cook had been able to make leucocyte counts; but he had not been able to discuss matters with Dr. Cook, because the papers were sent to him by post, and he had no information as to the exact way in which the films were prepared. He did not think that any trypanosomes had been found in a single case, because Dr. Cook would have mentioned that fact. In reply to Dr. Hill's question, he ventured to suggest that the reason why relapsing fever had disappeared was the progress of sanitation and cleanliness. He was exceedingly grateful for the kind words which had been said with regard to the paper, but the whole credit was due to Dr. Cook, who had been so good as to send him the results of his very arduous researches.

# TROPICAL TRICHOPHYTOSIS.

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(*Friday, July 17th*, 1908.)

Our knowledge of tropical dermatomycoses is far from being complete, and some confusion prevails in the description and classification of such affections even in quite modern standard works of dermatology. This is due to the scant opportunity there is of observing such diseases in temperate climates, and to the fact that as soon as patients move from a tropical into a cold climate the clinical features of these eruptions change greatly.

Having been interested in the subject during the last five years, I venture to give you the results of my investigation on the principal tropical dermatomycoses which I have had an opportunity to observe.

The dermatomycoses which can be classified under the heading of Tropical Trichophytosis are the following :—

1. *Tinea cruris* or " Dhobie itch."
2. *T. albigena* (Nieuwenhuis).
3. *Tinea of Sabouraud.*
4. *T. imbricata* (Manson).
5. *T. intersecta* (Castellani).
6. *T. nigro-circinata* (Castellani).

In addition to these affections, which may be considered as " Tropical " in a strict sense, several other forms of trichophytic affections occur in the Tropics which clinically are identical with the trichophytic affections met with in temperate zones, viz., *Tinea circinata, T. capitis,*

*T. barbae*, etc. These later affections are outside the scope of this paper, as their clinical features are identical with what one finds in temperate zones, though it is quite probable that the fungi are different species.

## Tinea Cruris.

### (*Dhobie itch, Tinea inguinalis*).

As you well know, the term "Dhobie itch" is applied very loosely in the Tropics by the lay public to practically any form of pruritic eruption. The term, however, is specially used to denote a form of severe pruriginous affection which mostly affects the inner surface of the thighs, occasionally the axillæ, and, in stout women, the regions under the breasts. It is in this stricter meaning that the term is used by medical men practising in the Tropics.

The clinical feature of the affection corresponds to Hebra's "eczema marginatum." In a well-marked case, the perineum, scrotum, and the inner surface of the thighs present large festooned patches with an abrupt elevated margin. The whole of the patches are bright red, or in a later period the margin only is red, while the rest of the patch is fawnish, or even normal in colour. The pruritis is unbearable. Owing to the scratching, secondary pyogenic infection, or an eczematous-like dermatitis, may develop. The complaint, if not properly treated, is extremely chronic; the condition gets better during the cold season, but recommences during the hot months. Patients who suffer very severely from dhobie itch may get almost well in a few days without any treatment on going to the hills; in coming back to the plains the pruritis and all the other symptoms reappear. The affection has been known to last for many years. It is to be noted that after a time the eruption may spread to other parts of the body—the abdomen, the trunk, legs, etc.—and may develop in rings or

may form solid elevated dark red patches. In such situations the disease may be clinically indistinguishable from ordinary *T. circinata*. It is to be noted, however, that the rings have generally a thicker edge and the patches are coarser and larger than in ordinary *Tinea circinata*. It is to be noted also that in my experience mixed infections of dhobie itch and ordinary *T. circinata* do occur presenting the two fungi growing on the same patient, though on different regions of the body. Several such cases I described some years ago (see *British Medical Journal*, November, 1905).

*Etiology.*—For many years it has been known that dhobie itch is a trichophytic affection. In 1905, as the result of an investigation of numerous cases, I came to the conclusion that *Tinea cruris*, or dhobie itch, should be separated from the ordinary forms of *Tinea corporis*.

*The Fungus.*—In my experience there is more than one species of trichophyton which may give rise to dhobie itch: at least two species can be distinguished, and there are probably several varieties of each :—

1. *Trichophyton cruris* (Castellani, 1905).
2. *Trichophyton perneti*.

*Trichophyton Cruris.*—This is the commonest species. In Ceylon about 98 per cent. of the cases are due to it. Sabouraud has created a new genus for this fungus—the genus *epidermophyton*. Notwithstanding Sabouraud's great authority, it seems to me that there is no necessity to create a new genus, as the fungus presents all the principal characters of the other trichophytons of the megalosporon type. Sabouraud lays stress on the rapid degeneration which takes place in the forms found in the cultures, and other secondary characteristics which seem to me to be of insufficient importance to necessitate the creation of a new genus.

The fungus is abundant in recent cases; it is extremely scarce in cases of old standing. The mycelial tubes in fresh cases are generally straight, have often a double contour, and the segments are rectangular, their breadth being $3\frac{1}{2}$ to $4\frac{1}{2}$ microns; branching is not infrequent; the spores are rather large, roundish, and present a double contour; they never collect in clusters. In chronic cases degeneration forms of the fungus are met with: the mycelium may be banana-shaped, presenting several constrictions; or long strings of ovoid elements may be seen.

The fungus is best grown by using maltose media. In cases of old standing, owing to its scarcity in the lesions, it is grown only with great difficulty. The material to be inoculated must be taken from the edges of the lesion. The growth begins to be visible after five to nine days; the rate of growing being very slow. After three to four weeks the colonies are about one half inch and even more in diameter. The colonies are orbicular-shaped, hard, with a pulverulent surface. The colour at first is greyish-yellow, with occasionally a greenish tinge; later it becomes a greenish white.

I have tried inoculation experiments on man with pure cultures of the fungus, but without any success. Sabouraud has also failed to reproduce the disease.

*Trichophyton Perneti.*—The fungus is microscopically indistinguishable from *Trichophyton cruris*: the cultural characters, however, are quite different, the fungus growing much more rapidly and the cultures presenting a delicate pinkish colour, which is generally lost in sub-cultures. This variety is found very rarely: clinically the lesions caused by it are identical with those caused by *Trichophyton cruris*. I proposed for the fungus the name *T. perneti*, as Pernet was the first to grow this species from a case of tropical trichophytosis. It was found, however, in only 2 per cent. of my cases.

*Communicability.*—*Tinea cruris* is known in the East as dhobie itch, from the popular belief that it is contracted from linen which has been contaminated while being washed by the dhobie. As to how far this belief is correct, I am not in a position to say. I have never succeeded in finding the fungus in clothes just received from the dhobie, either microscopically, or by inoculating small portions, of the linen itself in sugar media. I am, however, inclined to think that the popular belief may be to a certain extent correct, as a very few spores would be sufficient to set up the disease. I have been told by old sufferers, who had frequently been reinfected, that on their discontinuing to give their clothes to the dhobie, and having them washed in the house instead, the disease did not appear again. In Colombo dhobies are in the habit of washing the clothes in the lake or in small pools of water that are more or less stagnant; and it is certain that clothes belonging to infected persons are washed together with other clothes. Dhobie itch is very contagious: true epidemics occur in schools and among soldiers in barracks. I have several times observed that husbands infect their wives.

*Diagnosis.*—The diagnosis is easy in recent cases, the festooned appearance of the eruption limited by a sharp elevated bright red edge being quite typical In old cases, especially when secondary lesions, due to scratching are present, the diagnosis may be very difficult, the affection being often mistaken for eczema. A case sent to me had been previously treated as eczema for three months by Lassar's paste, calamine lotion, etc.

In doubtful cases, the microscopical examination will be of great help. It must, however, be noted that in old cases the fungus may be extremely scarce, the mycelium being practically absent, and only a few spores being found. It is well to take the scrapings for microscopical examination from the *edge* of the eruption.

*Differential Diagnosis.—Erythrasma.*— In erythrasma the patches have a fawn or dark reddish colour, with a fine desquamation; the eruption is not limited by a raised red edge, the fungus (*Microsporoides minutissimum*) is quite different from the fungi found in dhobie itch.

*Intertrigo.*—Intertrigo is very common in the Tropics, especially in corpulent persons. The lesions are very superficial, have not a festooned contour, and the margin is not sensibly elevated; no fungus is found.

*Eczema.*—Primary eczema of the scrotum and the skin of the thighs in contact with it, is as frequent in the Tropics as it is in temperate zones. The eczema is generally of the moist variety—the moist surface, the absence of the festooned elevated margin, etc., easily distinguishing it from *Tinea cruris*. As already stated, however, an eczematous-like dermatitis due to scratching often develops after a time on old dhobie itch lesions.

*Prognosis.*—The affection, if not energetically treated, has a tendency to become very chronic, and to last for years. Occasionally the eruption spreads to the whole body, forming rings or solid patches: at other times a distressing dermatitis develops on old dhobie itch patches, due to scratching. *Tinea cruris* may disappear during the cold season or when the patient goes to the hills, to reappear as soon as the hot season commences. During the period of quiescence the skin of the affected regions often shows a brownish discoloration, furfuraceous, somewhat similar to erythrasma.

*Treatment.*—In tropical practice the usual treatment is a chrysarobin or Goa powder ointment. All the so-called dhobie itch ointments sold as patent medicines contain Goa powder. The result is generally fairly successful. The patient should be informed that the medicine stains the linen, and gives also a dark stain to the skin, and that

unless carefully used unpleasant absorption may take place.

My usual line of treatment is as follows :—

*Very Mild Cases.*—A resorcin salicylic ointment applied twice daily.

*Cases of Medium Gravity.*—In such instances I use a chrysarobin ointment. In more obstinate cases I use local applications of turpentine oil in the morning, and at night a resorcin salicylic ointment. If the parts are much inflamed, at night simply a boric ointment or pack. This treatment gives good results. Turpentine is generally well borne, but patients often complain of a smarting and burning sensation, a quarter or half an hour after the application. Exceptionally one meets with patients who cannot stand turpentine.

*Severe Chronic Cases.*—In such cases I have found out, after many experiments, that the best treatment is that which I have introduced for *Tinea imbricata*, viz., resorcin dissolved in Tr. benzoin Co, to be applied once daily for several days. After some days the skin peels off and the eruption is generally healed. The only drawback to this treatment is that the medicine is very sticky, but it does not smart, and is generally well borne.

*Cases Complicated with Eczematous Dermatitis and Fissures.*—In such cases one is apt to use a soothing treatment by pastes, lead lotions, etc., with the idea to heal first the eczematous lesions. In my experience, however, when the microscopical examination has shown the presence of the fungus, it is best, as a rule, to start an energetic antitrichophytic treatment at once. To the fissures which so often develop in the inguinal regions, I apply a solution of nitrate of silver. I touch with this solution the fissures and all the moist parts; this application is somewhat painful, but the pain soon disappears, and the itching is relieved almost immediately. At night I apply a resorcin and sali-

cylic ointment or paste all over the eruption. As soon as the parts have become less moist, I start the turpentine or the resorcin-benzoin treatment. A precaution which must be observed during and after treatment, in order to prevent re-infection, is to dust all the under-garments with antiseptic powder, such as acid boric and zinc oxide, acid salicl. and talc, menthol powder, or dermatol.

### Tinea Albigena.

This trichophytosis was first described by Nieuwenhuis in Java, and his researches were subsequently confirmed by Jeanselme in Indo-China and in Siam (in the latter country the disease is known as *Khi*). The disease is fairly common in the Malay Archipelago, and is occasionally met with in Ceylon. It generally affects the soles of the feet and the palms of the hands, but may extend to the forearm and legs; it may affect the nails. The eruption begins with the appearance of small pruriginous spots on the palms and soles, the epidermis becomes raised and large vesicles develop containing at first clear serum; the bullæ burst, and the skin remains dry and peels off; the parts remain tender and there is desquamation and pruritis. A process of diffuse keratosis develops, the palms and soles becoming double their usual thickness; deep fissures may be formed at the natural folds. Several horny, semi-detached discs can often be seen at the dilated orifices of the sweat glands. The affection is very chronic, and it may begin in youth or adult life. After some time a process of apigmentation of the skin sets in—white patches, leucoderma-like, developing and extending often to the legs and arms.

As regards treatment, Tr. iodine and chrysarobin ointment (1 to 5 per cent.) answer fairly well.

*The Fungus.—Tinea albicans* (Nieuwenhuis, 1907) is a trichophyton of the megalosporon type; the spores in fresh

preparations from scrapings are almost always absent. The mycelium tubes are straight, occasionally showing a double contour; they are often dichotomous. Nieuwenhuis has grown the fungus, using Sabouraud's sugar media; the growth is very slow, the colonies are whitish and show a powdery surface.

The third tropical trichophytosis is the so-called

### Sabouraud's Tinea,

which was first described by Sabouraud in patients returning from Indo-China, Japan, and Tonkin. I have seen a few cases in Ceylon. The eruption generally commences on the uncovered parts of the body, ordinarily on the legs. The patients often state that they think the disease is due to prolonged immersion in stagnant water. The affection begins with erythematous patches, the surfaces of which are covered with minute pityriasis-like scales. Reaching, after a time, a diameter of one or one and a half inches, these patches become circinate. The circination, however, is incomplete; it is only segmentary. In dependent positions large polycyclic patches may be seen, but each forms only a half or a third of a circle, the rest of the arc being badly defined. The base of the patches, at this stage, is of a very dark bistre-brown colour. The border shows polymorphic lesions, mostly fine scales or minute vesicles and papules. The pruritis is very marked, and excoriations due to scratching are constantly present.

In chronic cases a thickening of the skin with lichenification takes place, especially at the circinate borders. The disease is difficult of cure in the Tropics, though it may disappear spontaneously on the patient proceeding to Europe. Chrysarobin ointment (1 to 4 per cent.) is the best treatment.

*The Fungus.—Trichophyton Sabouraudi* (Castellani,

1905).—The fungus cannot be grown on Sabouraud's medium or any other media I have tried. The mycelial tubes do not show a double contour, are not very straight; they are often banana shaped. The segments of the mycelium are all separated; the mycelial spores are roundish and are shed without forming a filament by their union. They are of various sizes.

### Tinea Imbricata (Manson).

*Historical-Geographical Distribution.*—The first reference to this disease is found in Alibert's Atlas, 1832. Fox in 1844 described it under the name of "Gune." He observed it in the Gilbert Islands, and "Gune" was the term used by the natives to indicate it (Gune = skin.) It is to Manson that we owe the first scientific description of the disease and the discovery of its fungus: he introduced the term *Tinea imbricata*, under which the disease is now generally known. Manson's researches have remained classical. Further researches were made by Turner, Königer, MacGregor, and more recently by Tribondeau, Nieuwenhuis, and others. The disease is extremely common in Fiji and many other of the Pacific Islands: it is found in the Philippine Islands, in Southern China, Tonkin, Malay Peninsula, and Java. Lately the distribution of the disease has much extended: several cases have been seen by me in Ceylon and Southern India, in which countries the affection was believed to be non-existent. Cases have also been reported from Brazil and other parts of tropical America.

*Clinical Signs.*— In a well-marked case the skin of practically the whole body is covered with round patches, each of which presents several concentric scaly rings. The scales are flaky, resembling tissue-paper, dry, of a dirty greyish colour, and slightly curled. If the scales are removed, rings of concentric circular dark lines remain visible. The number of rings forming the patch varies.

Tribondeau states that they are not more than four; I have, however, frequently counted as many as eight and ten in the same patch. The eruption may spread to any part of the body except, in my experience, the scalp. Though several authors state that the eruption never affects the face and axilla, and rarely the palms and soles, I have often observed it in such situations. It is also stated that the nails are not affected, though Manson, who first completely described the disease, said that the nails being affected was of frequent occurrence. My experience tallies with that of Manson. The nails when affected are much thickened, have a rough surface and deep cracks; scrapings examined in Liq. potass. show the fungus. The fungus does not invade the hair follicles.

The general health is not affected, but the patients complain of the disfigurement and of the pruritis. In the hot season the pruritis is much more marked. The disease is very chronic and very difficult to cure.

As regards the development of the disease, I can confirm Manson's description, having made several inoculations. The period of incubation varies between seven and ten days. After this time a slightly elevated brown patch is seen at the place of inoculation; the epidermis in the centre of the patch gives way and a ring of scaling epidermis is formed; this ring expands eccentrically and in the centre again a dark patch appears, which later breaks and another ring is formed inside the first, and so on.

*The Blood.*—The blood shows a certain degree of eosinophilia, the eosinophiles varying between 6 and 16 per cent. In some cases this eosinophilia was probably partly due to the presence of intestinal worms; the eosinophilia, however, was observed in some cases in which the microscopical examination of the fæces did not show any ova of worms. In very old cases the eosinophilia is more marked than in recent ones; in old cases signs of anæmia may be present.

*Prognosis.*—The general health is not affected. The patient chiefly complains of the disfigurement, which is very great, and of the pruritis, which in the hot season may be unbearable. The disease has no tendency to spontaneous recovery. Treatment is difficult, and cases apparently cured often show a return of the eruption after a few months.

*Diagnosis.*—*T. imbricata*, once seen, cannot be confused with any other dermatomycosis: the development in concentric rings fringed with tissue-paper like scales is absolutely characteristic.

*Etiology.*—The affection is caused by a trichophyton first discovered and described by Manson.

*Trichophyton concentricum* (Blanchard, 1901): syn. *Trichophyton mansoni* (Castellani, 1905).

*Description of the Fungus.*—In all the cases observed by me, the morphological characters corresponded with those given by Manson, rather than with the description of later observers. The fungus is very abundant, and it grows between the epidermis and the rete Malpighi. Fresh preparations of scales in Liq. potassæ show a diffuse mass of interlacing mycelium. The segments of the mycelium vary greatly in length; their breadth is between 3 and 4 microns. They are generally straight and of regular outline, without swellings or constrictions; the spores are rather large (4 to 5 microns), oval or somewhat rectangular. Aspergillar fructifications have never been observed by me, and I have never succeeded in growing the fungus. Strong very recently informed me that he also had been unable to grow the fungus in the Philippine Islands. According to Nieuwenhuis, however, the fungus may be grown on various sugar media. The latter observer states that the growth is slow and that the colonies are thick, crateriform, of a darkish colour. By inoculating the cultures in a European he reproduced the disease.

According to Tribondeau, the fungus is not a trichophyton: he calls it *lepidophyton*, from the aspergillar-like fructifications, but he has not succeeded in growing it. Although Wehmer has confirmed Tribondeau's observation, I repeat that in my experience the fungus never shows aspergillar fructifications In old scales kept in the laboratory for several weeks I have seen aspergillar-like fungi, but I believed them to be contaminations, as they are frequently found in old blood films, in old psoriasis scales, etc. It is not rare in the Tropics to find similar fungi in the skin of normal persons, especially when they do not bathe frequently.

*Treatment.*—All medical men practising in the Tropics know how difficult is the treatment of *Tinea imbricata*: it is easy to obtain temporary improvement—even disappearance—but as soon as treatment is discontinued, the eruption starts afresh.

In the Colombo clinic I made experiments to test the efficacy of various medicaments by simultaneously applying different liniments, ointments, etc., to symmetrical parts of the body and then comparing the result. The drugs employed by me were numerous—sulphur, white precipitate, turpentine, etc. These are my results:—

*Sulphur* has no effect whatever on the fungus.

*Turpentine* generally is slightly beneficial, some scales disappearing, and the skin becoming smoother; the improvement, however, is not permanent, and as soon as the turpentine application is discontinued the typical scales reappear.

*Calomel, white precipitate*, and other ointments of mercurial salts preparations do not induce any improvement in the eruption.

*Thymol* and *naphthol* ointments may cause a slight improvement.

*Carbolic acid* ointments have no effect whatever

*Cyllin* ointment (25 to 50 per cent.) may induce a temporary improvement.

*Formalin* is very effective for localised patches. The usual 40 per cent. solution is carefully applied to a small portion of the eruption. Formalin, however, often causes severe pain, and a certain degree of inflammation which is best relieved by applications of iced water. Soon after the application of formalin the patches turn dark brown, the colour lasting for a few days, and ultimately becoming clear. Care must be taken not to apply the formalin too freely or to repeat the application too often, otherwise a peculiar form of apigmentation, similar to leucoderma patches, may appear later on—a disfigurement to which coloured patients strongly object.

*Tr. Iodine.*—Tr. iodine freely applied induces a very marked improvement, which, however, is not permanent. Strong liniment of iodine, as recommended by Manson, is most efficacious.

*Chrysarobin.*—The repeated application of chrysarobin ointment (gr. 30 to one ounce) may induce a strikingly rapid improvement in cases which are not of long standing; in my experience, however, the eruption recommences a few days or weeks after its apparent disappearance.

Chrysarobin is a toxic medicament; the patient must be watched and the urine regularly examined. In one of my cases symptoms of absorption appeared after a single application.

*Salicylic acid* and *methylsalicylate* have very little, if any, action on the fungus.

*Resorcin.*—Resorcin, alone or mixed with salicylic acid in alcoholic solution and ointments, has very little efficacy. If, however, resorcin be dissolved in Tr. Benzoin Co. (grs. 30 to 80 of resorcin to one ounce of the Tr. benzoin), very good results are obtained, and this is now my routine treatment for *Tinea imbricata*. It is to be noted that

Tr. benzoin without resorcin has very little action on the eruption. I generally apply resorcin dissolved in Tr. benzoin once or twice daily to the affected parts. If the whole body be affected, one half is painted on one day; next day the other half is treated, and so on alternately. This treatment must be continued for several weeks, and once or twice a week the patient is given a very hot bath and is scrubbed all over with sand soap. In this way I have treated five cases, all of whom left the clinic apparently cured. After four months I had an opportunity of seeing three of them again. Two remained well; in one, two small patches had reappeared on the right shoulder. So far, I have not observed symptoms of absorption; in fact, the patient who showed signs of absorption after chrysarobin stood the resorcin treatment well. It is always prudent, however, to proceed with care at first, as it is well known that individuals may be met with—though rarely—showing idiosyncracy for resorcin.

In conclusion, it would appear from these researches that the best treatment for *Tinea imbricata* is Linimentum iodi., as suggested by Manson, or resorcin dissolved in Tr. benzoin, as suggested by myself.

## Tinea Intersecta.

I first described this affection at the International Congress of Dermatology held in New York, September, 1907, when I based my description of the disease on two cases. Later, I have come across several more instances among natives of Ceylon—Singhalese and Tamil.

The eruption begins with small oval, or roundish, slightly elevated itching patches, generally situated on the arms, legs, chest and back. The margins of these dark spots are slightly elevated and dotted with minute dark papules. At first the patches are dark brown in colour—darker than

the surrounding skin—and present a smooth tense surface; they increase slowly in size and some of them coalesce. After a certain time the surface of the patches becomes somewhat wrinkled; superficial cracks appear as white lines intersecting the brown surface of the patches; later, they become deeper; the epidermis splits, and flaky curled-up scales, whitish inside and dark on the outer surface, are often seen: these scales are removed by friction, when whitish round patches remain.

The eruption never develops in concentric rings like *Tinea imbricata*, the patches remaining isolated or fusing together to form irregular large areas. After a time, a few of them may disappear spontaneously. The general health of the patient does not seem to be affected. In some instances there is a slight degree of eosinophilia, which, however, may be due to intestinal worms.

*Transmission of the Disease.*—I caused one of the patients to scratch with his nails some of his patches and then to scratch a healthy Tamil coolie (who volunteered for the experiment) on the arms and upper part of the chest. On the arms nothing developed; on the chest at the inoculated place there was—on the second or third day—much itching; and on the fourth day two tiny dark, very slightly elevated, roundish spots appeared. These enlarged slowly, and a few days later their surface showed several whitish cracks. Unfortunately, at this stage, the coolie, who complained of great itching, scrubbed himself thoroughly with sand soap, and the eruption disappeared.

*The Fungus.*—If a portion of one of the brown patches or a scale be removed and examined in Liq. potassæ, the fungus is easily detected. It grows between the superficial and the deep strata of the epidermis, and is present on the inner surface, but not on the external aspect of the scales.

The organism presents the general characters of a trichophyton; but what is very remarkable is the extreme

rarity of free spores; in fact, in several cases I did not succeed in finding any spores at all.

The mycelium is fairly abundant, though far from being as plentiful as in *Tinea imbricata*. It is composed of long straight articulated threads, which are sometimes dichotomous, the breadth being between 3 and 3½ microns. No aspergillar fructifications nor clusters of spores can be seen, and so far I have not succeeded in growing the fungus.

*Differential Diagnosis.—Pityriasis Versicolor.*—When the eruption is in the very first stage it might be mistaken for a form of tropical pityriasis versicolor. In pityriasis, however, the epidermis does not split; moreover, in *Tinea intersecta* the fungus is not found on the surface, it grows between the superficial and deep layers of the epidermis.

### Tinea Imbricata.

This affection begins somewhat similarly to *Tinea intersecta*, with dark brown patches, and the fungus in both eruptions growing between the superficial and deep layers of the epidermis. In contrast to *Tinea imbricata*, however, the eruption of *Tinea intersecta* never develops in concentric rings; it is far less severe, as patches often heal spontaneously, and it is cured without much difficulty. I have had at the same time, in the clinic, cases of *Tinea imbricata* and *Tinea intersecta:* the two eruptions could not possibly be confounded.

*Treatment.*—Tr. iodine and the usual antiseptic ointments answer well.

### Tinea Nigro-circinata.

I have recently come across two cases of a peculiar trichophytosis which has not yet, to my knowledge, been described. Both patients were Singhalese. The disease in

one of them affected the scrotum and the neck; in the other the scrotum only. The patients complained of pruritis at the seat of the eruption, but the general health was not impaired. In both cases the features of the eruption were identical: there were a few rings with thick elevated margins, the encircled skin being black—much darker than the healthy skin surrounding it, but not thickened or presenting papules or vesicles; the edge, as already stated, was thick, much elevated, of a dark colour, with the upper portion pinkish or occasionally covered by a dark crust.

As regards the course of the disease, in one of the patients it healed without any treatment, leaving dark patches at the previous seat of the lesions. In the other case, Tr. iodine was used.

*The Fungus.*—On examining in Liq. potassæ scrapings from the edge of the patches, a fungus with the characters of a trichophyton was found in both cases. The spores, which were extremely rare, were roundish, rather large (4 m.) with a double contour. The mycelial tubes were straight, also with a double contour; occasionally they appeared to be dichotomous. I did not succeed in growing the fungus, either by Sabouraud's or any other media.

I suggest for this fungus the name *Trichophyton ceylonense*, and for the affection the term *Tinea nigro-circinata.*

## Tinea Rosea.

I may also describe here a new dermatomycosis which has been called *T. rosea*, although it is not due to a trichophyton, but is caused by a fungus of the genus *Malassezia.*

*Tinea Rosea.*—This dermatomycosis is fairly common among Europeans. It generally appears on the chest, axillary regions and back, and never involves the face,

The eruption consists of roundish or oval patches, not elevated or only very slightly so, several of them occasionally coalescing to form large patches of irregular outline. The colour is pinkish, or reddish-pink, occasionally coppery with a fawnish tinge; there are no scales, the surface is smooth and tense, rather shiny, or very finely wrinkled. As a rule, there is no desquamation; there is also no pruritis, or, if any, it is very slight. The superficial lymphatic glands are not enlarged. The blood does not show—at least in the cases I have examined—any abnormality. The general health is in no way impaired.

*The Fungus.*—If one of the reddish patches is scraped and a preparation examined in Liq. potassæ, the fungus can be easily detected. It consists of mycelium and spores, the general characters being those of a *Malassezia.* The mycelium closely resembles that found in Pityriasis flava and in the pityriasis of temperate zones; but the spores have more resemblance to the spores of *Tinea mansoni.* The mycelial threads are rather thick and often show swellings, constrictions, and other irregularities of shape. The free spores are globular, of rather large dimensions, 4 to 6 microns. They are often collected in clusters. I have not, so far, succeeded in growing the fungus.

*Diagnosis.*—The condition with which the disease is most easily confounded is a form of Seborrhœa corporis, localised on the chest, very frequent in hot climates. In fact, two of the patients sent to me were thought to be suffering from Seborrhœa corporis, and without the help of the microscope I should have made the same diagnosis. The microscopical examination will also distinguish the disease from the Pityriasis rosea of Gilbert.

*Ringworm.*—In contrast to ringworm, *T. rosea* is practically non-pruriginous; it does not develop in circles, and the margins of the patches are not sensibly elevated.

*Syphilis.* — On superficial examination the eruption,

especially when it presents a coppery colour, might be confounded with a syphilide, and several patients thought they were suffering from syphilis. The absence of other signs and the microscopical examination will clear the diagnosis. Of course, *Tinea rosea* may be seen in syphilitic subjects, and in such cases microscopical examination alone can render the diagnosis exact.

*Treatment.*—The eruption is easily cured: a salicylic-alcohol lotion (2 to 4 per cent.), followed by a resorcin and salicylic ointment (10 per cent. resorcin and 2 per cent. salicylic acid), generally gives good results. Tr. iodine also answers well.

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## Discussion.

Dr. Pernet said with regard to the variety of trichophyton, with which the author had been good enough to identify his (Dr. Pernet's) name, that some years ago when examining in this country dhobie itch cases which had come from India, China, and South Africa, he had twice obtained cultures of a rosy pink growth. He had been able to obtain several specimens of *Tinea imbricata*, but although he had tried a good many media he had utterly failed to get a satisfactory cultivation of that variety. It was obvious that in tropical ring-

worms of all sorts contaminations were often unavoidable, and a pure growth, especially in the media usually employed, was very difficult to get; but perhaps some medium would yet be found to grow *Tinea imbricata*. Dr. Castellani's able paper would be of very great value to dermatologists in this country. He had also obtained a pale pink culture from a case of *Tinea tropica unguium*. The disease had occurred in a patient from China, and he had directed attention to this variety of infection in the *British Journal of Dermatology* in 1906.

Fleet-Surgeon COLLINGWOOD said that he had seen cases of dhobie itch on the China station. The men were supplied with linen bathing-drawers, which were washed on board, apart from the ordinary washing, and there were no more infections. For treatment, ship's yellow soap, made into a creamy paste, had been used, and it was apparently very effectual, although in some cases he was afraid it had removed the skin as well as the parasite.

Mr. CANTLIE said that there was a somewhat similar disease prevalent on the coast of China, which was commonly known as foot tetter. It commenced by the formation of a bleb on the sole of the foot; the bleb ultimately broke, and, by and by, hard, bare, shelly flakes of skin formed and extended all over the sole and between the toes. Sometimes there was intense itching. The disease occasionally extended to other parts of the body; the navel, the back of the hands, and the axilla were frequently affected, and it was sometimes seen at the margin of the anus and between the legs. It usually died away in winter, but returned in summer; in fact, it came regularly with the hot weather in the Tropics, and even at home it recurred in summer for ten or twenty years after one had settled in this country. So far as he knew, the

disease was incurable; at all events, he had been unable to cure it, although various remedies had been tried. If a plaster were kept on for a time, when it was taken off a healthy skin was seen on the foot, the disease appearing to be quite cured, except for a little patch. From that patch, however, the disease recurred. He had tried to make cultivations in various media, without success. He hoped, however, that the investigation of this apparently incurable affection would be thoroughly undertaken.

Dr. CARNEGIE BROWN said that he could throw little light of scientific importance upon the difficult subject of tropical dermatomycoses, but he had observed more than one peculiar feature in the method of transmission of dhobie itch. In the first place, infection was possible only when the skin was already inflamed. Dhobie itch did not develop on uninflamed skin; there was necessarily a precedent erythema or intertrigo before the fungus would begin to grow, and that explained the sites of *Tinea cruris*. One very seldom, in fact, never, saw dhobie itch in any place except in the axilla, in the folds of the groin, in the perinæum, and between the nates. Dhobie itch, too, was inflammatory, while most other *Tineas* developed without concurrent inflammation. The microsporon infections of the scalp, the face, and the beard, such as the *Tinea* caused by trichophyton ectothrix, showed little or no inflammatory reaction to the growth. He should say that *Tinea imbricata* was also absolutely non-inflammatory. That eruption often looked somewhat angry, with red patches here and there in the earlier periods, but, doubtless, those were secondary infections; the disease itself did not set up inflammation or even hyperæmia of the deeper layers of the skin, while dhobie itch always did so. Another point was that, although the tropical physician saw this form of *Tinea* daily, almost the whole of his

patients were men—generally men who moved about a good deal, and who consequently perspired. Dhobie itch was seldom seen in women and children. Possibly that might be because the ailment was regarded as insignificant, and was dealt with domestically. But a personal experience of repeated infections of this annoying trouble led one to the belief that it was not spread by dhobies. Although the family linen was all washed together, skins that were presumably more tender, and, therefore, more susceptible, than that of the adult male were seldom or never affected. Dhobie itch was, indeed, very rarely seen in the nursery, and he could not agree that wives were frequently infected by their husbands. With regard to treatment, in the Tropics intertrigo around the scrotum was almost a normal condition; and if these parts were vigorously treated with a strong parasiticide such as chrysophanic acid, the application would certainly cause trouble. Goa powder and its preparations ought to be used with caution; he had seen serious results follow careless use, and absorption was sometimes attended by severe fever and other constitutional symptoms. Still, the fact remained that Goa powder was used as a cure for dhobie itch twenty times for once that other remedies were applied, and, if lightly rubbed in, in small quantities and with discrimination, it was safe. It was, indeed, the best remedy. The fungi of the various mycoses were difficult to grow upon the ordinary media; he had never completely succeeded with any of them. He wished the author had given more practical information on this point. No doubt the specific fungus grew, but there was always such a prolific growth of staphylococcus and other bacteria growing through it, that it could seldom be differentiated He always read with a certain amount of scepticism the descriptions of pure cultures of any microbic growth obtained from scrapings of skin in the Tropics.

Dr. F. M. SANDWITH said the cases he had seen of dhobie itch, although they might perhaps have been mild in character, had generally yielded to the application of Vleminckx's solution. He noticed in the list of fungi which Dr. Castellani had given that mycetoma was mentioned. He (Dr. Sandwith) wished to ask him whether Madura foot was prevalent in Ceylon, whether he had seen the varieties, red, yellow, and black, and whether the disease invariably occurred in the foot. When he first saw Madura foot in Egypt he did not recognise it, but Mr. Milton correctly diagnosed the second case, and since then many patients had been admitted to hospital. In one instance there was an opportunity for a *post-mortem*. As treatment by amputation was usual, it should be noted that in that case mycetoma extended up the tibia. There was also at the Tropical school a photograph of mycetoma in the groin. He had never seen or heard of a similar case; so far as he knew, the disease was usually confined to the foot, although it was rarely seen on the hand and breast in India.

Dr. GRAHAM LITTLE inquired whether Dr. Castellani used the same test medium in all examinations, and whether the separation of the various classes of *Tineæ* was founded on cultural characteristics only? He also wished to ask whether the various *Tineæ* were found in domestic animals, and whether the author had had any opportunity of observing for how long the infection could be carried by the scales. Was it, for instance, possible to take scales from patients for the purpose of observation at home? and would the fungi be found alive some months after the specimen had been taken from the patient?

The PRESIDENT congratulated Dr. Castellani on being one of the most successful investigators of epiphytic skin disease in the Tropics. He hoped the author would con-

tinue these investigations, and enlarge for publication the paper he had read. It was very difficult to realise from a verbal description the appearance of a skin disease, and if the subject could be adequately illustrated it would be of great advantage. The question of the animal origin of a number of skin diseases was interesting. Certain skin diseases in this country were connected with similar diseases in the lower animals, and it was quite possible that in the future it would be discovered that those of the Tropics were similarly related to domestic and other animals. He had considerable scepticism with regard to the value of cultures. He believed that occasionally a reliable culture was obtained, but one could hardly expect, under ordinary circumstances at all events, to get a pure culture from a skin scale. If scales from *Tinea imbricata* were put in a sterilised bottle and kept for a week, they became covered by an extraneous growth, showing that the skin was contaminated by some additional fungus to that which caused the disease. The only way of preserving, without actually destroying, the fungus of *Tinea imbricata* was to dry the scales, place them in a bottle also carefully dried, and then to hermetically seal it. In that way the fungus could be preserved for an indefinite period, but otherwise the results of cultures must be very carefully discounted. The cheapest, the most rapid, and the method best appreciated by the native for the treatment of *Tinea imbricata* was to paint him with iodine liniment, as strong as it could be obtained. A man with a thick skin did not appreciate mild treatment; if he felt the remedy acting on the skin with a certain amount of pain and burning, he thought an efficient drug was being used, and the efficacy of these drugs as a rule was in proportion to their irritating qualities. It was necessary to confine the paint to one half of the body at a time; care should be taken also not to cover the whole of a limb

with strong iodine. He had thought it would be an economical way of curing his coolie patients to put them in a sulphur bath, in which one after another could soak for half an hour or an hour, or even for two hours. He used a big tub with a solution of sulphuret of potash in hot water. The effect of it was to remove the scales of *Tinea imbricata,* but to leave the skin covered all over with the brown lesions which represented the attachments of the scales—in fact, the underlying fungus which was not killed. In his experience, sulphur was of no value whatever in the treatment of *Tinea imbricata.* For the treatment of *Tinea cruris* he used Vleminckx's solution—a solution of sulphuret of calcium. He diluted the solution in the proportion of one part to three parts of water; that was used on the first night, and washed off next morning. The second night the eruption was bathed with half strength solution, which was again washed off next morning; and the third night it was bathed with full strength solution, and this was allowed to dry on. That, as a rule, stopped the itching, and very often, for the time being at all events, caused complete disappearance of inflammatory trouble. The eruption often returned when the weather became warm, and if the patient was careless about cleanliness; but he found the method a good one, and it did not soil the clothes as chrysophanic acid did. He recognised Mr. Cantlie's description of foot tetter, which he used to treat by putting the patient's feet in a bath of carbolic lotion for half an hour. If the feet were allowed to get thoroughly soaked with a 5 per cent. carbolic acid solution, the disease was sometimes cured.

Dr. CASTELLANI, in reply, agreed with Dr. Pernet that dhobie itch originated, not in one fungus only, but in two and probably more species. *Trichophyton perneti* was not frequently seen in Ceylon, but it would be interesting to

note which of the two fungi were the more common in India, China, and other tropical countries. It would probably be found that in some countries *T. perneti* was as frequently met with as *T. cruris*. He also agreed with Dr. Pernet's remarks as to *Tinea imbricata*. Although he had tried various methods he had never succeeded in growing the fungus. He was therefore somewhat surprised at the results of Tribondeau and others, who had not only grown it but had reproduced the disease in man by means of cultures. Mr. Cantlie had given a vivid description of a very interesting tropical affection of the foot. He did not think that similar cases were seen in Ceylon, but instances of a peculiar eczematous dermatitis, characterised by terrible itching, and by the formation of large blebs and deep fissures on the feet and toes, were not uncommon. The treatment was very difficult; patients never got well unless they went to the hills or returned to their homes, but the method suggested by the President was certainly worth a trial. Carbolic solution would kill secondary germs, although probably pyogenic bacteria had nothing to do with the affection. Dr. Carnegie Brown had stated that, in his opinion, dhobie itch was contracted only when the skin was irritated. There was no doubt that infection was much easier when the skin was slightly inflamed than when it was in a normal condition. He thought, however, there were cases when the lesions of dhobie itch were not preceded by any marked inflammatory symptoms. Dr. Brown had also very justly called attention to the fact that in the Tropics medical men were apt to label every skin disease affecting the scrotum dhobie itch, and he certainly agreed with him that the use of chrysarobin ointment or strong goa powder should be restricted to cases where the diagnosis had been proved microscopically. Chrysarobin and Goa powder had a powerful destructive action on the fungus, but he had also seen very bad

effects from their absorption. He remembered the case of a young European who used one of the patent dhobie itch ointments, all of which contain a large amount of Goa powder. He applied it once only, but the day after he had erythema of the whole body with enormous œdematous infiltration of the penis and scrotum. His urine contained albumen and casts for several days. He also agreed to a certain extent with Dr. Carnegie Brown's remarks as to cultivation, and thought that many of the so-called pure cultures were very impure indeed. There was no doubt, however, that by using proper laboratory methods it was possible to grow some of these fungi. In reply to Dr. Sandwith's questions as to mycetoma, that disease was found in Ceylon, although it was not so common as in the south of India, but in the clinic for tropical diseases they always had two or three cases. The varieties of mycetoma obtained in Ceylon were the yellow and the black; so far he had not come across the red species. Mycetoma was by no means confined to the foot; he remembered a very interesting case, that of a woman who had typical and simultaneous infections in one foot and in the right mamma. The breast was removed and the streptothrix in it and in the foot were absolutely identical. Clinically, also, the symptoms were the same. In reply to Dr. Graham Little, he had always used Sabouraud's medium, and at the same time ordinary agar. In his last article on Trichophytosis in the *Journal of Parasitology*, Sabouraud said that, while on sugar media the fungi presented a high degree of pleomorphism, in media which did not contain sugar, pleomorphism was practically absent. Of course, in media devoid of sugar the growth of vegetable parasites was scanty, but for the purpose of comparing various species of trichophytons—at least tropical trichophytons—it was necessary to use

ordinary media besides sugar agar. He could certainly say that in the Tropics various forms of trichophytosis were found in the dog, and in Ceylon, as well as in this country, in the horse, cat, etc. He was not in a position to say definitely whether the special trichophytosis he had recently described, affected animals. He did not think, however, that *Tinea intersecta* was of animal origin. In the lower animals he had never seen a similar trichophytosis, but it was quite possible that future investigation would show that the *Tinea intersecta* as well as *Tinea imbricata* might be of animal origin. The difficulty of sending scales from the Tropics to Europe was that vegetable parasites, such as aspergillus and penicillium, developed on them. Sir Patrick Manson was the first to undertake the scientific study of these tropical skin diseases, and he (Dr. Castellani) could confirm his statement that the best treatment for *Tinea imbricata* in natives was strong liniment of iodine, while he thought the next best was resorcin dissolved in tincture of benzoin. In conclusion, he desired to express his thanks to the President and the other speakers for their most flattering remarks, which would serve to encourage him to continue his investigations.

# NOTES ON PARASITIC PROTOZOA OBSERVED IN AFRICA.[1]

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The present communication is an abstraction of the second part of a paper on the parasitic protozoa, other than pathogenic trypanosomes, observed by the late Dr. Dutton and myself during Expeditions of the Liverpool School of Tropical Medicine to Senegambia and to the Congo Free State.[2]

The parasites, for convenience, are classified according to the host in which they were observed.

In a Gambian dog a parasite resembling the hæmogregarine described in India by Christophers[3] was once seen.

In the blood of a grey hawk (*Asturinula monogrammica*) shot on the Congo, trypanosomes were seen. With them occurred an extraordinary number of parasites, resembling *Leucocytozoön ziemanni*, Lühe,[4] in various stages of development.

It is to be regretted that our material and opportunities did not permit us to make a thorough study of the latter parasite. Some interesting appearances were, however,

[1] Read at a meeting of the Society of Tropical Medicine on July 10, 1907.

[2] The first part of this completed paper appeared in Memoir XXI of the Liverpool School of Tropical Medicine, the second part in the *Annals of Tropical Medicine and Parasitology*, vol. i, No. 3.

[3] Christophers, *Leucocytozoön canis*, "Scientific Memoirs of the Officers of the Medical and Sanitary Department of the Government of India." New Series, No. 26, 1905.

[4] Mense, *Handbuch der Tropenkrankheiten*, vol. iii, p. 170.

noted. Male and female gametocytes similar to those described by previous authors are frequent; with them occur many interesting developmental forms.

In the large gametocytes it is noticeable that the nucleus of the host cell practically never takes the elongated form described by other authors in similar parasites. The general structure of the parasites of each sex at all stages of development is the same. Each parasite has periplast, ectoplasm, endoplasm and nuclear bodies. The periplast consists of a non-granular, pink-staining sheath, which invests the whole of the parasite. In it run fairly numerous, faint, non-staining, longitudinal, or, if the parasite is twisted, apparently diagonal striations (these can be seen by careful examination in even the youngest parasites). The periplast with its striations is, as a rule, best seen in the more lightly staining microgametocytes. In the adult forms the periplast is prolonged at either extremity into long, slender processes. When the matured parasite becomes rounded, preparatory to conjugation, the periplastic sheath is thrown off together with the effete host-cell nucleus.

Appearances in young parasites, as well as in the gametocytes, whose effilated extremities have a distinctly granular structure, indicate that an ectoplasm exists quite distinct from the periplast.

The endoplasm is the most striking part of the whole parasite. So much so that it, with the nuclear bodies, has been described by some as the whole of the parasite. In the macrogametocytes the endoplasm stains a deep blue. It frequently contains large numbers of coarse, or fine, vividly chromatophilic granules and many small, circular, non-staining areas or vacuoles. The endoplasm of the microgametocytes stains much less deeply, and rarely contains granules or vacuoles. The endoplasm appears to consist of a system of wide alveoli filled with a more or less

granular protoplasm. The structure is particularly well seen in the macrogametocytes. The sexual variation in the endoplasm exists, though in a minor degree, in some of the smallest parasites (endoplasm measures 4 $\mu$ by 3 $\mu$).

The amount of chromatophilic material present and the form of the "nuclear bodies" varies enormously, according to the sex and stage of development of the parasite.

A very constant phenomenon in parasites of all sizes is the presence of a long line of nuclear material, sometimes extending into the ectoplasm, which suggests a resemblance to some of those nuclear changes described by Schaudinn in a parasite of this species.[1]

Every stage can be observed in the development of these adult sexual forms from very small spherical parasites which probably are able to wander from host cell to host cell by amœboid movement. Forms which might be considered definitely as intermediate between the leucocytozoa and trypanosomes do not occur.

In two hornbills (*Bycanistes buccinator*) trypanosomes of a common type were seen. They are interesting in that their periplasts show definite longitudinal striations; their blepharoplasts are granular, and in several preparations two small, darkly-stained and closely-opposed, chromatophilic granules, surrounded by a pinkish area, occur in close connection with the nucleus, but just outside of the nuclear membrane.

In a Gambian puff-adder an extraordinary, attenuated trypanosome was seen. In the blood of the same snake were numerous drepanidia. These parasites are of a usual type, but they are accompanied by curious rod-shaped bodies, staining homogeneously by Romanowsky's method, of unknown nature. They measure from 10 $\mu$ to 19 $\mu$ in

[1] Schaudinn, *Generations- und Wirtswechsel bei Trypanosoma und Spirochaeta*. Arb. a. d. Kaiserl. Gesundheitsamt. Bd. XX, Heft 3, p. 434.

length and 1 $\mu$ in width. They occur singly or in pairs, and are usually intracellular.

In a crocodile, and in a tortoise, trypanosomes and drepanidia both occurred.

The blood of several hundred frogs and toads was examined in the Gambia and in the Congo. *Trypanosoma loricatum* was present in them, and from a study of, for the most part, fresh films, the following cycle of development was made out[1]:—

The well-known, oblong trypanosome becomes rounded and loses its flagellum. Certain ill-defined, nuclear changes then take place and the rounded parasite divides into two halves. Binary division proceeds until the single, original parasite has become a clump of forty or more small, round bodies, closely resembling a group of agglutinated white cells. After a short period each of these cells acquires a flagellum. Then the body of each becomes ovoid, then pear-shaped, and at last the whole parasite becomes herpetomonas-like in form. Binary division often occurs still among these primitive flagellated forms. From observations on stained specimens, it is thought that it is possible to trace the development of these herpetomonas-like flagellates through a series of intermediate forms back to the original typical *Trypanosoma loricatum* from which we started.

*Drepanidia* and *Cytamœvæ* frequently occurred in the frogs in which the cycle of development just described was observed. In addition to these a few small intracellular parasites were seen, which could not be identified. With the latter were associated bluish-green (Romanowsky stain), intracorpuscular, crystal-like bodies.

In a scaleless fish (*Clarias angolensis*) in the Congo trypanosomes were seen. These are interesting in showing

[1] Danielewsky, *La parasitologie comparée du sang.* Darre, Kharkoff.

a distinct, quadruple granulation of the blepharoplast, two of the granules in binary division going to each of the resulting trypanosomes. In the blood of the same fish occurs a previously-undescribed spirochæte ; for it the name *Spirochæta jonesii* is proposed.

An interesting coccidium was seen in both adult and larval mosquitoes. In tsetse flies a parasite probably belonging to the *Myxosporidia* was frequently seen.

Although the facts presented in this communication are admittedly the results of passing observations, incomplete, disconnected, and made under unfavourable circumstances, we think that they possess a certain value as a record alone of protozoan infections encountered by chance during three years' work in Africa. Many of the appearances noted, though incomplete in themselves, are of additional value in that they confirm the constancy of morphological changes noted in other allied and better studied parasites.

The following examples are cited. Longitudinal striations have been seen in every trypanosome examined ; the myonemes of other authors are, therefore, constant in the trypanosomes we have dealt with. In several instances the thickened edge of the undulating membrane of trypanosomes has been observed to terminate, not in the blepharoplast, but in a lighter staining pinkish granule, or pair of granules, closely applied to it. In some of our trypanosomes the structure of the nucleus and the occurrence, in neighbouring differentiated areas, of paired chromatophilic granules suggests a connection with the complicated nuclear developmental phenomena observed in trypanosomes by some observers. The clear, unstained, perinuclear area, the irregular, non-staining, almost granular areas, and the lines in the cytoplasm as well as the chromatophilic granules frequently observed in the various trypanosomes examined are all constant phenomena, which seem to be of biological significance. In the development of *T. loricatum* the work

of Danielewsky is confirmed and extended to indicate a most interesting cycle of multiplication in a well-known trypanosome; it must be asked whether a similar cycle may not occur in other trypanosomes. In the leucocytozoön of birds an interesting course of development is described.[1] Very little has been said concerning the drepanidia observed. In spite of a very careful search in kept preparations of blood, we were unable to find any indication of a direct relation between this parasite and the trypanosomes which were often associated with it. Neither were parasites resembling *Trypanosoma inopinatum* seen to become intracorpuscular.

One of the earliest of the lessons learned from our work on these protozoa was the entire inadequacy of the methods of preparing blood-films, ordinarily employed by pathologists, for a morphological study of protozoa. The examination of dried films stained by modifications[2] of Romanowsky's method suffices for the purposes of clinical diagnosis. This method will show many of the structures present. It was used in the whole of the work described in the present papers, but it fails to demonstrate finer details properly. Our work must, therefore, be regarded as incomplete, and certainly the more important parts of it should be repeated by workers using methods more perfect than those employed by us.

A second lesson was that too much of the work done on the pathogenic protozoa, particularly by medical men, has been directed by conceptions derived from bacteriology.

Days of tedious searching of slides and of observation of

[1] The dangers of constructing a part of the life-history of a parasite from stained specimens alone are apparent. The course of development we described may be mistaken, but the description of the forms mentioned are accurate and may be relied upon, so far as the defects of the method of preparation employed permit.

[2] Stephens and Christophers, *The Practical Study of Malaria*, p. 10. University Press of Liverpool.

parasites, placed under various conditions, must be spent in studying the pathogenic protozoa, when minutes would almost suffice in the case of bacteria. The difficulty of finding the parasites at all is sometimes extraordinary, and the possible occurrence of a latent infection must never be forgotten.

The study of the pathogenic protozoa must be approached with an unbiased mind and with the remembrance that the known life-cycles of several protozoa are exceedingly complicated. We believe that the continuous observation of living parasites will ultimately furnish the richest reward. A single positive observation so obtained is absolute, and outweighs any number of apparently antagonistic probabilities obtained by deductions from work done along apparently parallel lines of research. Of course, the examination of fresh preparations should be supplemented by the study of stained material. Lastly, at the present moment, more is known, in every way, of malaria than of almost any other disease. In observing less studied protozoan infections it will frequently happen that our knowledge of what actually does occur in malaria will lead to the formation of an ultimately successful working hypothesis, or to the correct interpretation of newly-observed phenomena.

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